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Mrs. Mary A. D. Scharlieb, M.D., President.

DISCUSSION ON THE CAUSES AND TREATMENT OF DYSMENORRHOEA.

OPENING PAPER.

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The frequency with which I have put my views on this subject before the profession has the advantage that I can excuse myself for speaking in what might seem too dogmatic fashion by referring to former papers as containing the reasons for what I say; while if those who take part in this discussion happen to have read my former papers,[1] their criticisms will at least not be hasty.

The first question that arises is, What is dysmenorrhoea? Some take it to mean any kind of pain or discomfort when the patient menstruates. They say that it is a symptom, and that for its treatment the cause must be discovered. If it means this, my comment is that our discussion would range over the whole field of disease in woman; there is scarcely any disease of any part of the body that may not, as one of its manifestations, cause some increased trouble when menstruation occurs.

I hold that there is but one disease to which the term "dysmenorrhoea" ought to be applied. This disease is one *sui generis*. It consists in terribly severe spasms of uterine colic at the time of menstruation. It is often associated with absence of sexual feeling and sterility in marriage. It is in most cases cured by dilating the cervical canal, by which treatment often sexual feeling is produced and sterility cured.

According to the best numerical estimate that I can form, rather more than a third but less than a half of English women menstruate without pain. About one woman in eight is accustomed to lie up during part or the whole of the time that she is menstruating. But not every woman who lies up when she menstruates suffers from dysmenorrhoea. Discomfort of which a strong woman will take no notice will prostrate a weakly one.

Patients with dysmenorrhoea usually suffer from more than one kind of pain. Beside the spasms of dysmenorrhoea they have the general lassitude and pelvic aching that precedes and accompanies menstruation in all but the fortunate minority. But even if the sufferer had at the beginning no other pain than that of dysmenorrhoea, yet when the terrible spasms of pain have recurred month after month for years, the resistance of the central nervous system to pain becomes reduced, and reflected pain is felt over the area supplied with sensory nerves from the tenth, eleventh, and twelfth dorsal, and the first lumbar, segments of the spinal cord. There is tenderness over the skin supplied from these segments.

The characteristic features of the pain of true dysmenorrhoea are its sudden onset, its short duration, its extreme severity, and its not being relieved by lying down. Sometimes the pain suddenly wakes the victim up at night. I have known the pain only to last ten minutes, but in this brief period it was enough to make the patient vomit. It is exceptional for it to last more than twenty-four hours. It is not relieved by lying down. Patients' friends often describe how they roll about and writhe with pain. Often the patient vomits, sometimes faints. There is hardly any disease of the pelvic organs which causes pain so severe.

The extreme severity of the pain is one of the best criteria of genuine dysmenorrhoea. At the time when Mackintosh practised he had neither anaesthetics nor antiseptics, and operative treatment was not so freely advised or so readily accepted as at the present day. I think that we may assume that the young women who before 1836 allowed Dr. Mackintosh to dilate the cervical canal in order to relieve their monthly pain almost certainly suffered severely. Dr. Mackintosh[2] up to 1836 had thus treated 27 patients, 24 of whom were cured. I doubt if the records of any gynaecologist of the present day could show such a large proportion of cures. The reason is that anaesthetics and antiseptics have so removed the objections to local treatment that it is now employed in many cases, not because the operator is sure that it will do good, but because he thinks it may do good and knows of nothing else that will. This is a good reason for dilating the cervix, for it is a harmless procedure, which leaves the patient none the worse. But a series of cases selected on this principle is sure to include many which are not dysmenorrhoea, but merely monthly pain or discomfort due to psychasthenia or neurasthenia. In the early years of the nineteenth century young women would suffer a good deal before they would submit to a painful operation on the cervix uteri, and their mothers would have had to be convinced that the suffering was real before they would have let them incur the risk; so that I think Mackintosh's cases were carefully selected, hence his large proportion of cures.

There is a kind of dysmenorrhoea which writers of textbooks are almost unanimous in accepting, which in my judgement does not exist, namely, "obstructive dysmenorrhoea." Cases occur of obstruction to the outflow of menstrual fluid; but such obstruction does not produce pain so severe as the terrible spasms of dysmenorrhoea. Pain in this condition is usually slight; sometimes there is none. The condition generally lasts for months or years before the patient's mother or guardian thinks it necessary that she should see a doctor; and then it usually is because menstruation has not arrived, although the external signs of puberty have long been present. Sometimes the doctor is consulted because a swelling in the abdomen has been noticed, but hardly ever on account of pain. The fact that there is pain usually has to be elicited by questions.

Membranous dysmenorrhoea is another thing which I hold not to exist. The passage of a membrane from the uterus during menstruation is common. Robust women pass them without pain. Pain accompanying the passage of a membrane is usually an indication of neurasthenia or psychasthenia. Such pain is hardly ever so severe as that of true dysmenorrhoea. When in a patient who passes membranes with pain the tone of the nervous system can be restored, by rest, plenty of food and sound sleep, she will pass membranes without pain. In abortion, the substance to be passed is larger than a menstrual decidua, and there is pain; but the pain is seldom severe. The patient usually sends for a doctor because the haemorrhage alarms her, not because the pain is intolerable. The passage of a membrane each month is commoner than dysmenorrhoea. But dysmenorrhoea may afflict a patient who passes a membrane each month. In such a case it is possible for a woman to be cured of her dysmenorrhoea, and yet go on passing membranes. Membranous dysmenorrhoea is usually only a manifestation of neurasthenia or pyschasthenia.

Dysmenorrhoea depends not upon the size, shape, or position of the uterus, or upon the size of the cervical canal, or the os externum. It occurs when the uterine canal is straight as well as when it is bent. The angle in the canal, and the dilatation of the uterine cavity behind it, which at one time were shown in diagrams in many textbooks, have never been found to exist in an actual section of a uterus. I have seen cases, and so have other people, in which the os externum was round and so small that an ordinary uterine sound could not be passed through it, but there was no dysmenorrhoea. In some of the worst cases of dysmenorrhoea the os externum is of ample size, and the sound can be passed easily.

Many other kinds of dysmenorrhoea have been described. Gebhard[3] says the causes of dysmenorrhoea

may be disease of the tubes, the ovaries, the serous covering of these organs, or the pelvic connective tissue. These he would class together as "inflammatory dysmenorrhoea." Its characters are pain seven to ten days before menstruation begins, lessened when the flow appears, and ceasing when the flow has become free, a dull pain, a feeling of weight and fullness; in many cases there is also head and stomach pain.

This is a correct clinical picture; but it is not dysmenorrhoea. It is pelvic pain due to inflammation, made worse by the premenstrual vascular tension. It should be called by the name of the local morbid change which is present.

Among other things which, according to Gebhard's view, we should include under the term "dysmenorrhoea" are hysterical contractures and paralyses, psychoses of various kinds, disorders of the stomach, enlargement of the liver, jaundice, biliary colic, tuberculous disease of the lungs, heart failure, movable kidney.

Doubtless a monthly aggravation of the symptoms of these different maladies under the strain of menstruation has been observed. But this is not dysmenorrhoea. I think that nothing but confusion can result from calling such different conditions by the same name, even though qualified by different adjectives.

This, then, is the answer I give to the question, What is dysmenorrhoea? It is spasms of uterine colic, recurring with each menstruation. It is a disease *sui generis;* unlike any other. I think that there is an absence of the dilatation or relaxation of the cervix which ought to accompany contraction of the body, and these contractions are therefore violent and painful. There is no mechanical hindrance to the escape of blood. There is plenty of room for the patient to bleed to death if enough blood escaped from the vessels.

The next question we are invited to discuss is the *cause* of dysmenorrhoea. Much has been written about this, and many causes assigned. I know nothing about the cause or causes of dysmenorrhoea.

The test of knowledge is the power of prediction. Let me illustrate. If an oculist examines the eyes of a young girl and finds a high degree of hypermetropia and astigmatism, he can predict what will happen if she spends much time in reading without proper spectacles. But, given an apparently healthy young girl who has not yet menstruated, I know of no criteria which will enable any one to predict that this particular girl when she menstruates will suffer pain, or that she will not.

In about two-thirds of the cases dysmenorrhoea dates from the beginning of menstruation; in about one third it begins later. It is rare for it to develop after 25. Dysmenorrhoea is thus a disease of adolescence. If the patient is questioned closely enough as to what she thinks may have been the cause of her pain, she can generally recollect something on which she can put the blame. But I know of no antecedent so constant or so clear in its mode of action that I can explain by it the production of pain. I think dysmenorrhoea occurs chiefly in what are called "neurotic" people—that is, in the people with the most highly developed nervous systems; for, the more complicated a machine is the more easily it is put out of order. Although I think this general assertion is in harmony with clinical experience, yet I can formulate no criteria by which these people are to be distinguished from others.

Dysmenorrhoea has no tendency to spontaneous cure. On the contrary, in the words of Dr. Mackintosh, who wrote what I think must have been the first good description of the disease:

> These periodical sufferings, however slight at first, afterwards became more severe and of longer duration, so that at length some women are beginning only to recover from the effects of one period when the approach of the next is close at hand.

The explanation given by modern neurologists is that the spasms of pain felt along the nerves running from the uterus to the spinal cord so disturb those segments of the cord, that reflected pains are felt in regions supplied with sensory nerves from those segments. As the pains recur month after month the disturbance becomes more and more severe, is sooner and more easily provoked, and more slowly quiets down.

When dysmenorrhoea occurs late in life, the most common cause is a fibroid or fibroids.

The only natural cure for dysmenorrhoea is pregnancy. By the birth of a child the cervical canal is dilated to the fullest extent, the dilator being the child's head. I am told that among the negro population of the West Indies this is well known, and a girl suffering from dysmenorrhoea will be told by older women that she should have a baby. But the laws which govern social life among the more highly civilized races prevent this remedy being available for every patient, and sterility is often part of the disease.

I have now to discuss the *treatment* of dysmenorrhoea. The first proposition that I have to lay down is that every case of dysmenorrhoea can be cured. This has to be tempered with the admission that in many cases the cure is worse than the disease. The radical cure is to stop menstruation by removing the ovaries. This is a thing seldom to be considered. The thing that is desirable is to preserve menstruation but to stop the pain. From this point of view there are two aims in treatment: (*a*) The relief of the pain when it has arrived, and (*b*) to prevent the pain from coming.

First, (*a*) the pain, when it has arrived, can always be abolished by the hypodermic injection of sufficient morphine. The objection to this is the danger of setting up the morphine habit, which is a worse disease than dysmenorrhoea. The best analgesics are the coal tar derivatives—antipyrin, phenacetin, aspirin, pyramidon. I know of no harm from these drugs. But in the worst cases they do not relieve, and sometimes the patients vomit and cannot keep the drug down.

The general aching and malaise and lassitude that comes in weak patients from the raised vascular tension and reduced nervous tone that precedes and accompanies menstruation is not cured by anything except restoring the nervous tone, if possible, by food, rest, and sound sleep.

There is one drug which will often cure dysmenorrhoea—namely, guaiacum. The least disagreeable way in which patients can take it is in cachets. I prescribe in this form 10 grains of guaiacum resin to be taken three times a day. Sometimes this causes a little flatulence, colicky pain, and diarrhoea. If so, it is well to add to each dose 1 grain of pulv. ipecac. co. I have been accustomed to advise the patient to begin taking it a week before menstruation was expected. I doubt if it is necessary to begin it so long before; but I have thought it better to give the drug the best possible chance.

I cannot give numerical results on account of the difficulty of following up the cases. But I will relate illustrative reports:

One patient, a very intelligent lady, as will appear from the details she gives, first consulted me in 1895. She began to menstruate at the age of 17, but had no severe pain till the age of 21. Then she began to have pain, which she compared to "dagger thrusts." She was married at the age of 25. When she was 26 the cervical canal was dilated. After this she menstruated three times without pain, then the pain returned. At the age of 30 she became a widow. She was married a second time at the age of 34. In January, 1908, she consulted me again about her menstrual pain. She said it was "awful," and lasted four hours. I prescribed guaiacum. For the purpose of this discussion I wrote to ask her the result, and I epitomize her reply. She takes the guaiacum every month. The best results are always when she takes it a couple of days beforehand, though she only takes it once a day for those two days, unless she specially wants to make sure of being very well for some particular engagement, when she takes it twice a day. When she takes it three times a day she is ever so much better, and can almost say she has *no* pain at all. Being so much better she forgot about the medicine about six weeks ago, and had a very bad three hours. She has suffered, she thinks, "as much as anybody could do"; had tried nearly everything else, and had almost given up hope of being any better. She can now get about while menstruating if she has to; for instance, she went on the first day to see the late King's funeral procession, without any bad result.

I relate another case:

An unmarried patient, aged 21, I saw on March 1st, 1909. She began to menstruate at the age of 12; had always been regular, with a copious flow lasting six days. She suffered from slight aching a week before; severe pain for two days. Guaiacum was prescribed, as in the preceding case. In answer to inquiry, I have a letter dated June 6th, 1910. She says, "While I took the cachets I had no pain whatever." She has not taken any since July, 1909, and the pain has since been so slight that she has not thought it necessary to take them.

In another case which I quote for the sake of illustration, the report is:

My daughter still has rather a trying time; she says *the actual pain has not been so severe*, but the backache, lassitude, and general upset is not much improved.

This report clearly defines the two kinds of pain—the spasms of dysmenorrhoea, which were made less severe by the drug, and the reflected pain called out by the reduced resistance of the nervous system incident to the vascular disturbance accompanying menstruation which was not improved.

Guaiacum will not cure every patient. Even if each month when it is taken pain is lessened or abolished, yet the patient cannot be said to be cured so long as she is still dependent upon medicine.

The cure of dysmenorrhoea is by dilatation of the cervix. In the most complete form of the disease dilatation of the cervix will abolish the menstrual pain, develop sexual feeling, and cure sterility.

The best way of dilating the cervix is by metallic bougies, as recommended by Mackintosh. He used "metal bougies of different sizes, from that of the ordinary small silver probe to No. 14." I think the graduation of the bougies according to the English catheter scale is better than increasing the size by a millimetre in diameter, as in Hegar's dilators. The difference in size being less, the dilatation is more gradual and gentler. In dilating the cervix by bougies the force used is appreciable by the operator's hand. This is not so in the case of a tent. The tissue forming the os internum is sometimes so rigid that even the enormous pressure exerted by a laminaria tent, which Duncan found by experiment to be over 600 lb. to the square inch, fails sometimes to dilate it. The tent in such a case expands above and below the os internum; and surgeons have found themselves obliged to cut through the cervix in order to remove the tent.

I do not think a tent is in these cases the best way of dilating the cervix; but, if a tent is used, it should not be too thick a one. Two slender tents side by side are better than one thick one. Still less do I approve dilators, of which many have been devised, of composite blades made to diverge by a screw after being passed into the uterus. These instruments tear the cervix instead of stretching it. The operator with one of these weapons cannot measure the force he is using.

Dilatation with metallic bougies is, in my opinion, the best treatment. The next question is, How much dilatation? Mackintosh used no bougie larger than No. 14. In this he did more, rather than less, than was necessary.

The following case may be interesting on account of the long period during which the patient's history is known, and also as illustrating the amount of dilatation which is effective. The patient was born in 1851. She began to menstruate in 1867. Was married in 1875. Menstruation was always painful, laying her up every month, worse after marriage. In November, 1878, bougies 6, 7, and 8 were passed through the os internum without any anaesthetic. The dilatation was not carried further, on account of the pain it caused. With the next menstruation the pain, though not quite absent, yet was described by the patient as nothing compared with what it used to be. In December, 1878, bougies from 6 to 10 were passed. No. 10 was firmly gripped, and produced acute pain, said by the patient to resemble that of menstruation. I last saw this patient in the autumn of 1903. She then told me that menstruation had ceased in 1901. She had never been pregnant. She had had no menstrual pain since the last dilatation in 1878.

There are cases in which the os externum, instead of being a slit such as suggested to our forefathers the name "*os tincae*," is a small circular opening. The smallness and the circular shape do not denote dysmenorrhoea, for they have been observed in many women who menstruated without pain. Nor are such women sterile. But if a patient with such a small circular os externum becomes pregnant the membranes cannot effectually bulge into the small os externum, and we have a case in which labour pains go on for twenty-four or forty-eight hours or more without appreciable dilatation.

In the early Seventies the routine treatment in London of dysmenorrhoea was to divide the vaginal portion. Some did this bilaterally; some in one place only. In many cases this proved successful. It was dropped because cases were met with in which, after the vaginal portion had been divided without curing the patient, dilatation of the internal os did cure her. But the cases of small circular os externum are those in which it seems to me good practice to divide the vaginal portion as well as dilate the os internum, so that, should pregnancy afterwards occur, morbid prolongation of the first stage of labour may be prevented.

There is a more dangerous way of dilating the cervix, which is putting a stem into the uterine canal and leaving it there. The lower part of the vagina of every woman contains millions of micro-organisms of many kinds; and therefore, although it may be possible to make a stem or bougie aseptic for a short time, it is not possible to keep it so while its lower end remains in the vagina.

Every case of dysmenorrhoea can be cured by stopping menstruation; that is, by removing the ovaries. The danger from the operation is so small, and the suffering involved so slight, that it would be a delightful thing to be able to tell the patient that she could be cured so easily were it not for the very serious ulterior consequences. These are of course the loss of the potentialities of marriage and motherhood. This means the loss of the patient's greatest potential happiness. If the patient is young, she will, if rightly instructed, summon up all her fortitude and bear the pain as long as she possibly can. But when a patient has passed the age of greatest fecundity and has still no prospect of marriage; still more if she has reached the age (35) at which, if she marries, she is more likely to be sterile than to have children, then I think she is entitled if she chooses to ask to be relieved of menstruation and the sufferings attending it. She alone knows how much she suffers; and she alone can judge of the value to herself of what she is renouncing.

There are cases in which even at an early age it may be proper to stop menstruation. If a young woman has no relatives who can support her, has to earn her living, and cannot keep a situation because every month she is prostrated by pain, she may fairly claim to decide for herself, and, if she thinks fit, sacrifice the possibilities of marriage and motherhood.

In patients who have grown accustomed to be invalids for some days each month, being reduced to that condition by the dysmenorrhoeal spasms and the reflected pain which precedes and follows them, the segment of the spinal cord which was disturbed by the menstrual pain does not at once regain its normal tone. Reflected pain recurs at monthly intervals as before. But it gets less and less each month, and usually after six months is either absent or trifling.

Before advising oöphorectomy it is necessary to be quite sure that the case is one of dysmenorrhoea; that the pain has arisen out of menstruation and nothing else; that it is not merely a monthly manifestation of psychasthenia, neurasthenia, or hysteria, nor pain, such as biliary, renal, or intestinal colic, due to some local disease which independently of menstruation would cause pain. If the pain is not dysmenorrhoeal, removal of the ovaries will not cure it.

REFERENCES.

[1] *Obstetrical Transactions*, vol. xliv, 1902, p. 371, and BRITISH MEDICAL JOURNAL, April 17th, 1909. [2] *Principles of Pathology and Practice of Physic*, fourth edition, vol ii, p. 434. [3] Veit, *Handbuch der Gynäkologie*, Band iii, 1 Hälfte.

DISCUSSION.

Professor GOTTSCHALK (Berlin) said that in the treatment of dysmenorrhoea it was of great importance to recognize the cause. General treatment was indicated in cases of anaemia, hysteria, neuralgia, and general nervous debility, while in cases of tuberculosis vigorous tonic treatment and improved hygiene should be followed. In some cases with nervous debility cauterization of the septum nasale (Fliess) might be successful. In cases with increased blood pressure, oxygen and air baths might calm the pains. Aplasia of the uterus could produce dysmenorrhoea. A narrowing of the internal cervical os was a frequent etiological factor in mechanical dysmenorrhoea, as also was pathological anteflexion of the uterus and anteflexion of the cervix caused by deep insertion of the ligamentum sacro-uterinum on the cervix. In these cases local treatment was to be followed. The speaker obtained dilatation of the internal os of the cervix by means of laminaria tents increased in size on three successive days. On the fourth day the uterine cavity might be packed with iodoform or isoform gauze; this gauze should be removed at the end of forty-eight hours. He kept the patient in bed for six days. He believed that manual treatment would be the best to adopt

in dealing with anteflexed cervix. In some of his cases there had been great elongation of the cervix; here only the resection of the cervix could calm the pains. In one case of dysmenorrhoea membranacea he found numerous venous thrombi in the expelled membranous discharge. They had led to the bursting of the thin-walled capillaries at the fundus of the spongiosa by back pressure, combined with diffuse haemorrhage. The case was one of stagnating thrombosis, induced by functional weakness of the cardiac action. A corresponding general treatment (gymnastics, baths, diet) strengthened the heart, and cured the dysmenorrhoea membranacea, after curetting and intrauterine therapeutics had failed. The speaker believed that dysmenorrhoea membranacea was not necessarily inflammatory. Concerning the so-called "middle pains," he distinguished between a genuine, severe form and a pseudo, slight one. The real middle pains were induced by an intermediate ovulation at a time when the uterine mucous membrane was not yet ready to menstruate. Some time ago he had seen a 30 years old virgin with healthy genitalia who, since her puberty, had suffered regularly from severe abdominal pains, up to six days in duration, a fortnight after menstruation; the latter did not induce any pain. These spasms never appeared if a profuse intermediate haemorrhage of one week's duration set in a fortnight after menstruation. That happened, therefore, at a period when the uterine mucosa had loosened sufficiently to make menstruation possible. The slight form of "middle pains" was induced, without any intermediate ovulation, through the commencement of the menstrual wave and the growth of the mature follicle, either in women who were neurasthenic or hysterical, or in whom there existed an inflammatory sexual disease. Relief could be obtained by scarification of the cervix. These slight forms were curable, while the genuine middle pains only stopped when intermediate ovulation ceased.

Professor W. Nagel (Berlin) said that, unless he had misunderstood Dr. Herman, the latter denied the existence of membranous dysmenorrhoea as a separate disease, but the speaker held that cases were met with, though not very frequently, in which, under great pain, a fully developed membrane was expelled every month. He had seen a case of the type in which all kinds of treatment—including intrauterine faradization, dilatation, and curettage—had proved useless. He was compelled to extirpate the uterus, and by this operation the patient was at last cured. He would further ask him whether he had had any experience of nasal dysmenorrhoea, about which there was much discussion in the Berlin Obstetrical Society. Opinions as to the occurrence of nasal dysmenorrhoea differed very widely. It was, however, a fact that dysmenorrhoea and affections of the nasal mucous membrane occurred in the same individual simultaneously, and that when the nasal affection was cured the dysmenorrhoea would disappear.

Dr. Curtis Webb (London) said he was glad to hear Dr. Herman so strongly express the view that true dysmenorrhoea was of the nature of a spasm—a cramp of the uterine muscle. No doubt there were objective causes that might bring about this cramp, such as clots of blood retained in the uterus, but the speaker would solely refer to the menstrual pain where no such cause could be diagnosed. He acknowledged the suggestion made in the speech by Dr. Herman, and also in a previous publication of his, of the great benefit frequently derived from the administration of guaiacum resin, and said that he himself had often prescribed the drug with great advantage. He also referred to the *comprimes d'alcool* often employed in France to allay the pain at the menstrual period. His special wish, however, was to draw the attention of the Section to the excellent and permanent results that could be obtained in a very large percentage of cases of true dysmenorrhoea by the use of suitable electrical treatment. The fact had been long recognized in America and on the Continent, but in this country any reference to the use of electricity in gynaecology was anathema to the profession. In the cases of unmarried girls, where for obvious reasons local examination was undesirable, and operative interference only to be used as a last resource, the regular application of the static wave current, as described by Dr. Morton of New York, by means of a smooth, metallic, rectal electrode, coupled with abdomino-dorsal applications of the constant current—30 to 60 m.a. for ten minutes three times a week—would very often cure the tendency to irregular spasm of the uterus, and thus remove the pain. The treatment was painless and simple, but the writer laid stress on the absolute necessity of having an efficient static machine of ample power. (Details of the technique will be found in the British Medical Journal, May 1st, 1909.) Dr. Webb had employed this procedure in 13 cases of dysmenorrhoea in unmarried girls with only one failure; and he considered that the present prejudice against the use of electro-therapeutics in gynaecology should be put aside, and a fair trial be given to a recognized branch of treatment that recent advances had removed from the realm of empiricism to that of scientific therapy.

Dr. R. J. Johnstone (Belfast) welcomed the restriction of discussion to the spasmodic form of dysmenorrhoea, since the inclusion of all cases of painful menstruation would cover the whole field of gynaecology. To the definition of Dr. Herman, he would add that no definite and constant anatomic lesion was discoverable. The cause of the pain was spasm of the muscular tissue of the internal os uteri, and might be compared to the painful contractions of the sphincter ani in cases of fissure of the anus. The treatment of the condition should be very similar to that of anal fissure, and should be by means of dilatation of the canal. Some tearing of the fibres of the muscle was necessary for a permanent cure, and in many cases it was necessary to divide the cervix. This operation the speaker usually performed when a previous dilatation had failed to give lasting relief. There was also a form of dysmenorrhoea arising two or three years after puberty, and gradually increasing in severity, which was almost invariably associated with "cervical erosion." The only cure was the removal of the new growth, and the part of the cervix from which it grew.

Dr. Inglis Parsons (London) said that he preferred to take a wider view of the definition of dysmenorrhoea, because of its clinical importance. A case came into his wards that had been dilated several times without benefit; he found a diseased appendage, and after removal of this the pain disappeared. For purposes of diagnosis he divided the cases into occasional and persistent. The occasional cases were due to anaemia, neuralgia, and gout. When persistent there was always a definite cause. The principal causes were anteflexion of the uterus, diseases of the tube and ovaries, and fibroma. That some cases of anteflexion had no pain he was quite aware, and one patient of 35 with anteflexion came to him on account of sterility, but with no pain. Dilatation cured the sterility. On the other hand, the vast majority of cases were due to anteflexion; possibly the cervix failed to retract so as to allow the menstrual fluid to pass, and this failure caused the pain. His best results were obtained by a gradual dilatation under an anaesthetic. If there was any erosion, curetting was done and a glass stem inserted and left in for a fortnight while the patient was in bed. It was removed before the patient got up. A glass stem was quite innocuous in these conditions, he was quite sure. He knew of one patient who went out wearing a stem, against his colleague's orders, and she actually became pregnant while wearing it. One of the worst cases he had seen was due to a small fibroid in a young woman of 25, who implored him to remove her ovaries. A myomectomy was done and the patient cured. She subsequently married and had a child. Two similar cases had come to him, and both were cured by myomectomy. One of these was a woman of 35 who had two children, and the other was a young woman of 26. He recently showed a fibro-adenoma of the Fallopian tube, at the Royal Society, occurring in a tuberculous subject, where he had to remove both appendages. In this case and in all the others he had mentioned the only symptom complained of was dysmenorrhoea. With regard to membranous dysmenorrhoea, he could not agree with Dr. Herman that it did not exist. It was undoubtedly rare, but one case which had been in his wards used to pass a complete cast of the uterus about once in two or three months. When the membrane

was passed she had severe pain; when there was no membrane there was no pain. The treatment of occasional cases consisted in giving iron for anaemia and salicylate of soda or aspirin for gout. After a chill a hot bath with a purge and a mixture of carminatives produced the best results.

Dr. FREDERICK EDGE (Wolverhampton) considered faulty nutrition from the age of 7 to 17 the chief cause of spasmodic dysmenorrhoea, which was due, as Dr. Herman had pointed out, to uterine contraction, painful because ineffective and irregular, and opposed at the sphincter of the cervix. This defective muscular development was found in malformation and atrophy of the uterus. Girls should have three good meals a day. Discission, including internal division of the sphincter of the internal os, was successful in these cases of dysmenorrhoea. At the same time, examination for small myomata and other lesions could be made, and curetting would make sure that some small mucous polypus was not to blame.

Dr. BLAIR BELL (Liverpool) said that he could not agree with Dr. Herman's statement that there was only one form of dysmenorrhoea. To do that would be to admit that dysmenorrhoea was a disease, whereas it was a symptom of many conditions. He classified dysmenorrhoea into two large groups: (1) Disorders of the normal physiological conditions. (*a*) Exfoliation of the endometrium (membranous dysmenorrhoea). (*b*) Atony of uterine muscle, with the formation of clots *in utero*. (*c*) Deficient contractility of the uterine muscle, due to bad development of the uterus, or to contortions, such as acute anteflexion. In these uteri there was a ring of non-contractile tissue. (2) Acquired pathological conditions, such as salpingitis and fibromyomata. He considered that obstruction did occur, especially when the uterus was distended by clots or a membrane; in these circumstances, the pains were "labour-like" in character and lasted until the cervix had dilated sufficiently to allow of the escape of the uterine contents. If the cervix was very small, it should always be split anteriorly or posteriorly. The question of successful treatment was chiefly dependent on the recognition of the causal factors and their scientific treatment. He was strongly opposed to the removal of the ovaries, which was often followed by disastrous results to the mental stability of the patient; nor ought the uterus to be removed. Morphine and alcohol should never be given, lest a habit be established. Further work on the ductless glands—which so largely influence the menstrual function and development of the genital organs—would lead to important advances in the treatment and prevention of dysmenorrhoea.

Professor MURDOCH CAMERON (Glasgow) could not limit dysmenorrhoea to the spasmodic form. There was also a membranous form. The symptoms to which it gave rise were more severe and alarming than those of any other form of dysmenorrhoea. Anteflexion was the most common cause. He treated it by dilatation and the introduction of a stem pessary between two periods. In cases of sterility due to anteflexion this treatment was very effective. He entered a protest against removal of the ovaries.

Miss M. H. FRANCES IVENS, M.S. (Liverpool), thought acute anteflexion was the commonest cause of dysmenorrhoea, but that a great many cases were associated with extreme constipation, and were much improved by free purgation. For other cases dilatation under anaesthesia was in a very large proportion of cases successful.

The PRESIDENT (Mrs. Mary A. D. Scharlieb, M.D.) could not agree that spasm was the only cause for dysmenorrhoea. All present must have seen cases which appeared to be true obstructive dysmenorrhoea due to sharp flexions *plus* swollen mucous membrane, and also cases where a small polypus acted like a ball valve. As to membranous dysmenorrhoea, when the membrane was passed whole or in two large pieces it became rolled into a ball, which might be half an inch or more in diameter. This must necessarily cause pain when it excited muscular contraction strong enough to force it along the undilated cervical canal of a virgin. It was true that every case of dysmenorrhoea could be cured by arresting the function, but such arrest involved the removal of either both ovaries or the uterus. Conceivably, in cases of absolutely incapacitating dysmenorrhoea in a woman who had to earn her living, one of these operations might be necessary. Naturally no one would propose it until all other available means had been tried and had failed. If a major operation had to be performed, hysterectomy should be preferred to oöphorectomy, because the uterus was only the sac intended for the protection of the developing ovum, and was not, as far as was known, essential to the woman's health, whereas the ovaries were certainly of value, not only as subserving menstruation and conception, but also on account of their internal secretion, which appeared to exercise a profound influence over the general health of the younger women. It was certainly true that the general health was often profoundly disturbed by nasal and retropharyngeal troubles, and it was therefore not possible to deny that in some cases dysmenorrhoea might ensue, and be cured by nasal operations. The spasm of the circular muscular fibres surrounding the os uteri internum, to which Dr. Johnstone had alluded, and which he treated by division, might be treated by valerianate of zinc and belladonna pills. She agreed that the membranous dysmenorrhoea was usually accompanied by excess of loss due to the stripping off of the endometrium in bulk and not in the usual minute fragments.

Dr. HERMAN, in reply, said that the distinction between menstrual pain and dysmenorrhoea was important. Pain from anaemia, neuralgia, tuberculosis, pelvic inflammation, adenomatous growth, etc., might accompany menstruation, but these conditions were not dysmenorrhoea. Healthy women often passed membranes without pain. There were cases in which menstrual pain was so severe as to prevent young women from earning their living, and to make older women chronic invalids. Such cases could be cured by removing the ovaries. They also probably could be cured by hysterectomy. He had had no personal experience of the effects of hysterectomy for dysmenorrhoea; but he knew that oöphorectomy would cure it. It had been said that atrophy of the ovaries followed hysterectomy. As to this question, he had not been able to satisfy himself. There were cases in which it was necessary to stop menstruation; but there was room for difference of opinion as to the best way of doing this. Anteflexion he regarded as the natural shape of the uterus in most virgins; it would, therefore, be surprising if it were not found in cases of dysmenorrhoea. He had been much interested in the results of nasal treatment of dysmenorrhoea, but had not himself been able yet to come to a conclusion as to its value. Electricity, applied two or three times a week for three months, seemed to him a less simple treatment than dilatation, even if the dilatation had to be repeated. Intermediate pain he thought probably due to ovulation; but this, so far as he knew, was conjecture, he knew no proof. Stem pessaries were happily now little used, but in the past they had often caused fatal peritonitis.

THE STRUCTURE OF THE STROMA OF THE ENDOMETRIUM, AND ITS BEARING ON THE MENSTRUAL CHANGES.

By JAMES YOUNG, M.D., F.R.C.S.E.,

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THE stroma of the uterine mucosa is composed of an aggregate of cellular elements which are intimately united by protoplasmic communications with their neighbours. The orthodox conception regards it as a connective tissue of a primitive or an embryonic type, the intercellular spaces of which intercommunicate freely to form a vast and complex system filled with lymph fluid. Whilst the view just stated may, I think, be fairly taken to represent the commonest idea, Leopold has considered the stroma as constituted of two structurally distinct entities, a connective tissue network composed of branching and communicating fibrils, the meshes of which are lined by the stroma cells; these are flattened endothelial plates.

The researches here recorded in brief were carried out for the purpose of testing the validity of these conceptions. The results coincide with previous teaching in interpreting

the stroma as a poorly differentiated, embryonic tissue, but they differ from this in many points which are fundamental for a proper appreciation of the functional changes which the mucosa exhibits during menstruation and pregnancy. To-day I propose to refer more especially to the menstrual function.

The *blood vessels* of the mucosa in the resting state are lined by cells which are usually flattened like the endothelial layer in other regions. In vessels thicker than capillaries the media is composed of one or more layers of cells arranged concentrically. Immediately external to this is the ordinary stroma. Whilst the appearances at first sight would sem to justify the conclusion that in the formation of the vascular wall there has been a specialization of structure, I have been convinced by my investigations that such is not the case, and that the vessel wall is constituted, throughout its entire thickness, of elements structurally identical with those of the surrounding stroma. In support of this I would advance the following observations, all of which are figured in the plates and photomicrographs which I show.

1. In the first place it is a matter of common finding that in the premenstrual and the menstrual conditions the media elements become opened out by the escaping blood fluid. In some cases the cells become stripped off the wall in successive layers, the process naturally commencing on the outside. It progresses inwards until the entire circumference of the vessel wall becomes teased out. In other instances the opening out involves one part of the wall more than, or prior to, the other parts. In these circumstances it is found that the cellular elements forming intima and media are identical in every respect with those of the ordinary stroma. The intimal cells are seen to communicate freely by protoplasmic bridges with one another and with the adjacent medial cells. In many instances it is impossible to tell with certainty which were the original intimal, which the medial, and which the ordinary stroma elements.

A study of the changes during this teasing asunder of the vessel walls reveals, in addition, a fact of moment in regard to the structure of the stroma. This is found in the great ease with which the opening-out and displacement of the tissue can occur. So much is this the case that one is forced to the conclusion that the stroma must be looked upon as a soft, mobile protoplasm.

2. The second fact in support of the structural identity of the intima and stroma elements is found in the observation that, in many instances, the blood vessel wall, even in the resting state, is seen to be composed of tissue clearly similar to the ordinary stroma. In such instances even the apparent differentiation of structure is absent.

3. It is clear that if the description of the structure of the vessel wall which I have just given be correct we would expect to find an absence of the supporting tissue elements encountered in vessels in other regions. From the beginning it seemed to me certain that in this respect the endometrial vessels must diverge from the typical structure, and with the object of testing this belief I have carried out an extensive investigation. The results coincide with those of other workers with regard to the fibrous and muscular elements. In no part of the mucosa, except in the region immediately bordering on the muscle, are these tissues ever found to enter into the structure of the vessel walls or the stroma. With regard to elastic tissue, I have stained and examined seventy-five specimens of the mucosa. In no part except again in the very deepest regions is this tissue ever found. In the vessels, just after they reach the mucous membrane from the muscular coat, a fine elastic lamina just under the intima is not infrequently seen.

We can now understand how it is that the vessel walls can become opened out so readily. More than that, as structure subserves function, it would seem likely that this peculiar structure must be intimately bound up with the vascular gaping during menstruation and pregnancy.

4. To complete the evidence in favour of the structural identity of the intima, media, and stroma elements, it is necessary to refer to the discovery that during pregnancy the intimal and medial cells can enlarge in a manner identical in every respect with that associated with the decidual increase in those of the stroma. This change, as in the stroma, is accompanied by a proliferation of the intimal and medial cells.

The escape of the fluid constituent of the blood with a teasing out of the stroma and the vessel walls is such a constant precursor of the haemorrhagic infiltration of the tissues associated with menstruation that it was thought likely that any light thrown on the processes underlying the oedematous infiltration would provide us with a clue to the causation of the menstrual changes. The importance of the oedematous opening out of the structures is recognized when we remember that in the stage of the menstrual cycle which immediately precedes the blood flow, that is, the premenstrual phase, there is a widespread and extensive watery infiltration of the upper layers of the mucosa.

How does this watery infiltration occur? In considering this question we are brought face to face with the varying beliefs entertained with regard to the production of lymph and of oedema in general. Briefly stated, these fall under one or other of three headings:

1. That the fluid element of the blood escapes in response to the laws of filtration; that it is due to a mechanical squeezing of the fluid through a vessel wall from the lumen, where the pressure is higher, to the tissue space, where it is lower. Under this heading would seem to fall the orthodox conception regarding the production of the menstrual oedema and haemorrhage—namely, that they are due to a congestive process.

2. According to modern teaching, a fluid escape across the vessel wall is dependent, in addition to the factor just noted, on tissue changes which determine an osmotic diffusion from the vessel lumen. The establishment of the osmotic streams is induced by a liberation of crystalloidal elements in the tissue spaces, which are retained by the protoplasmic films, and the outward passage of fluid tends to continue until the osmotic pressures on either side of the fine membranes are equalized.

3. The third theory advanced to explain the transmission of fluid from vessel to tissue supposes the existence of a vital and selective secretory activity on the part of the vessel endothelium.

In the vessels from the mucosa of a uterus removed entire during menstruation the walls are markedly thickened; and I refer to this case in the first place because in it the stages of the process are forced into prominence. There is a marked vacuolar swelling of the intimal cells, which is clearly due to an active fluid imbibition. This is proved by the bulging of the cells towards the lumen. In some of the vessels, in addition to this fluid imbibition by the endothelial layer, there is present an escape of fluid into the surrounding tissues along tracks produced by a displacement of the vessel wall and stroma. In some instances these clear tracks lead directly from the intima into the surrounding tissues; in other cases they are interrupted by fine protoplasmic films which bridge across the oedematous space. The presence of these fine membranes, it seems to me, contraindicates the possibility that the fluid escape is due to mechanical causes. A squeezing of fluid from the vessel lumen sufficiently strong to tear up the surrounding tissues in the manner present could not leave these films undamaged.

The arguments just advanced against the mechanical explanation apply with equal force to the theory which traces the oedematous escape to a specific secretory activity of the lining cells.

So far, then, the structural changes suggest that the fluid escape is not dependent on mechanical factors, and the alterations in the intimal elements prove that, at any rate all along the inner aspect of the vessel wall, there is taking place an active fluid imbibition by the cell protoplasm.

That, under normal circumstances, the oedematous escape from vessel to stroma is due, not to a mechanical filtration, but to protoplasmic changes resulting in an active imbibition, is proved by a careful histological study of the mucosa in the premenstrual and the menstrual states.

The condition that I wish particularly to call attention to is the extraordinary tissue change associated with the fluid escape. The typical appearance of the stroma, in the shape of more or less well-formed cells united by comparatively simple protoplasmic processes, has become completely modified. Instead of the comparatively small number of fluid areas, the protoplasm is beset with a multitude of spaces which in some cases are so infinitely small as to be just recognizable under a magnification of 1,000, whilst in other places they attain dimensions as large as, or larger than, the ordinary intercellular space.

These facts show that the fluid which has passed into the stroma is contained not in freely communicating spaces, according to the orthodox conception, but in an immense number of chambers completely walled in on every side. In other words, we see that the intercellular bridges correspond not to filaments but to films of protoplasm. That such is the case was suggested to me by the observation that in most instances the confines of the spaces are complete and not interrupted as they would inevitably be, in most cases, if the sections were carried across tissue threads. This observation, then, would indicate that the explanation of the oedematous infiltration is to be found in the existence of an active imbibition by the stroma, and not in a filtration from the vessel lumen with a consequent mechanical displacement of tissue. That this is the correct interpretation of the changes is proved, in addition, by finding that the fluid increase is represented not only by an expansion of the intercellular regions, but that in the process the perinuclear cell protoplasm often becomes transformed into a number of fluid spaces, bounded by the displaced and thinned-out cytoplasm. This change implies, without doubt, an active fluid imbibition, and according to modern teaching (see Moore, Adami, etc.) it can be explained in one way, and in one way only, namely, that there has occurred a change in the cell substance associated with a liberation of crystalloidal elements and a consequent osmotic transference of fluid from the adjacent vessel or tissue space.

To understand the manner in which the tissue changes result in a continual stream through the vessel wall into the stroma, leading to the ploughing-up present, it is only necessary to appreciate the manner in which the fluid escapes from the intimal layer into the immediately external tissue. As already stated, the fluid enters the intimal cell in an endeavour to readjust the osmotic pressure discrepancy between the protoplasm and the blood. From this it would seem clear that the imbibition of the fluid results in a lowering of the osmotic tension of the lining cell. This establishes a discrepancy between it and the immediately external protoplasm with a diffusion of the fluid outwards. The fluid which it thus loses it must again imbibe from the blood vessel. These changes, advancing in an outward direction, will determine, so long as there are tissue bridges present, a continual stream of fluid. The fact that changes such as I have described are occurring is indicated by the observation that, with an opening out of the vessel wall, the swollen intimal cells in the corresponding region are invariably seen to collapse. The most markedly distended intimal cells are always in a vessel, or in that part of the vessel wall which exhibits the teasing out in the least degree. These facts indicate that the fluid imbibition of the lining cells is to be taken as a sign of the existence of such osmotic changes in the tissue. They also explain the influence of the condensation of the vascular walls in determining the degree of the intimal changes in the specimen referred to, and conversely why in the finer vessels with easily opened out walls the process is apt to be obscured. I have pointed out that the vessels in the normal stroma consist merely of tracks through a protoplasm which is easily opened out, and this, I believe, explains why the real nature of the changes in the endometrium during menstruation has hitherto evaded detection. When the intercellular spaces become enlarged and the cells are widely separated, it is often difficult to determine that this has occurred in any way but by a mechanical leakage of fluid into the tissues.

It would seem that the investigations just recorded provide us with an explanation of the structure and function of the intricate "network" which many authors have described in connexion with the endometrium. By Leopold it is held to be a structural entity independent of the cells and to constitute a scaffold on which the stroma is built; this idea would seem to commend itself to some more recent writers. By the majority of authors, however, this network is recognized as composed of finely-drawn-out fibrils in direct structural communication with the cell protoplasm.

As the result of the investigations which I have recorded in brief I would contend that we must recognize in this network, with its many and varying changes, an index of the intricate functional alteration for which the stroma is especially adapted. These appearances are not merely incidental, but are of profound importance from the point of view of a proper understanding of the changes in menstruation and in pregnancy. The considerations advanced suggest strongly that the network of previous writers in reality corresponds to the spaces in the protoplasm with their thinly-drawn-out but complete envelope of cytoplasm enclosing the fluid which has been actively imbibed. The presence of this complete shell of cell substance was an absolute certainty in the case of the swollen intimal elements.

In some vessels the bulging fluid vacuoles are richly scattered along the inner aspect of the vessel wall. The manner in which the cell protoplasm of the stroma swells up with the imbibed fluid, often to many times its original size, indicates beyond doubt that the water absorbed is contained within complete films of cytoplasm. These appearances, it seems to me, prove that the so-called network formed by interlacing threads of cell protoplasm in reality corresponds to an agglomeration in the cytoplasm of fluid which has been actively imbibed, and which is contained in spaces separated by complete walls. The fact that the fluid imbibition is exhibited by the stroma elements in a manner similar to that present in the intimal cells indicates that the alterations in these are to be considered more as an incident in, than the cause of, the increased fluid escape; in other words, that there is nothing to favour the recognition of a local secretory influence on the part of the lining cells.

The fact also that the protoplasmic changes during menstruation are found to be exhibited in an identical manner by all the different elements of the stroma (intima, media, etc.) furnishes additional confirmatory evidence in favour of the interpretation of the structure of the stroma which I have previously advanced. The lining and supporting cells of the vessels were seen to be *structurally* similar to the stroma elements. They are now seen to be *functionally* identical.

The escape of the fluid element of the blood into the stroma is due to a complex protoplasmic change resulting in an active fluid imbibition. How are we to account for the exodus of the red cells from the vessels into the adjacent tissues? Is it due to a mechanical leakage of the red cells into the stroma as far as this is opened out by the oedema, or is it not rather due to an active dragging out of the cells by the force which I have shown to determine the fluid escape? This, I believe, accounts for the process, and many of my sections would suggest that the red cells can pass bodily through the stroma protoplasm, leading to no more rupture or displacement of this than if it were a jelly-like medium.

SUMMARY.

The following statements summarize my conclusions:

1. The stroma of the endometrium consists of a soft semi-fluid protoplasmic mass imperfectly differentiated into cellular elements.

2. The cells anastomose freely with one another by means of protoplasmic processes. They present many and varying alterations in shape, but these are easily dispelled, and the cells then approximate to the typical stellate shape. The differentiation of the stroma cells is, thus, probably more apparent than real.

3. The intercellular spaces, in all probability, do not communicate directly with one another. The anastomosing processes are not, as is usually stated, filaments, but films of protoplasm, which under ordinary circumstances close in the fluid cavities.

4. The intima and media of the vessels are nothing more than ordinary flattened stroma cells. This shape they easily lose.

5. Except in the deepest layers of the mucosa the vessels have no specialized supporting coats (muscle, elastic tissue).

6. The vessels are obviously so constructed as to allow a ready and universal opening up of their walls, and the structure and consistence of the stroma such as to permit its ready displacement by fluid or blood.

7. The oedematous infiltration of the tissues, which precedes the haemorrhagic escape, is due neither to a mechanical displacement or filtration of fluid from the vessels, nor to a secretory activity of the intimal cells. It is dependent on protoplasmic changes, which result in an active imbibition of fluid from the vessels, by a process of osmosis. So far as we at present know, this change is due to a widespread liberation of crystalloidal elements in the tissues.

8. The infiltration of the stroma with blood corpuscles is, in all probability, due to exactly the same cause. In consequence of its peculiar structure, the uterine mucosa must be looked upon, throughout its entire extent, as a potential blood sponge.

In conclusion, I would state that my investigations have revealed that the peculiar structural conformation of the endometrium is intimately bound up with the changes

which occur during pregnancy. The vascular gaping round the young ovum occurs in a manner identical in nature to, and differs only in degree from, the mode in which the vessels open up during menstruation. These observations, and also their bearing on the function of the decidua, I shall shortly publish elsewhere.

DISCUSSION ON TREATMENT OF FIBROIDS COMPLICATING PREGNANCY.

OPENING PAPER.

By Walter W. H. Tate, M.D.

There has been a tendency among some gynaecologists and operating surgeons to overestimate the probable dangers to which a pregnant woman is exposed if afflicted with fibroid tumours. Others, perhaps, may be inclined to minimize the risks.

In order to arrive at a fair and trustworthy conclusion it is essential to consider the following points: (1) The unfavourable changes which may occur in fibroids during pregnancy; (2) the frequency of miscarriage and any special dangers arising from this accident; (3) the increased risk of labour, whether due to *post-partum* haemorrhage, septic infection, suppuration, or sloughing of the fibroid.

Risks from Changes in the Tumour.

We will first consider the unfavourable changes which may occur in fibroids where pregnancy coexists. It is obvious that the natural enlargement of the pregnant uterus when associated with a large fibroid will be a cause of discomfort, and may occasion serious distress. It is difficult in these cases to estimate to what extent increase in the size of the actual fibroid is responsible for the whole uterine enlargement. Some are of opinion that the enlargement is only apparent, others are of opinion that fibroids undergo oedematous infiltration during pregnancy. It is fair, however, to say that where the fibroid is of only moderate size before pregnancy and forms an abdominal swelling there is every reason to hope that it will not give rise to any serious distress from its size alone. When, however, a woman has a very large fibroid the chance of her becoming pregnant is small, but should such an event occur, it is quite possible that serious symptoms might arise in the latter half of pregnancy from upward pressure on the diaphragm, owing to the rapid increase in size of the uterus. Necrobiotic change may occur during pregnancy and may cause serious discomfort or persistent pain, which may make interference with the pregnancy necessary. It has been suggested by Donald that owing to rapid increase in the size of the fibroid during pregnancy, pressure on the renal veins with albuminuria may result. We know that enormous tumours, either ovarian or uterine, may be present in the abdomen without giving rise to any albuminuria, and it seems therefore unnecessary to suggest pressure on the renal veins to explain the symptom, as the albuminuria may have been the result of the ordinary toxaemia of pregnancy. So far as pressure symptoms are concerned the patient is undoubtedly exposed to serious dangers of this nature where the fibroid encroaches upon or is entirely situated in the true pelvis. It is easy to understand that where pregnancy coexists under these circumstances, pressure symptoms may occur at an earlier date than would otherwise have been the case. Torsion of the pedicle of a fibroid tumour during pregnancy is so infinitely rare that it need not be regarded as a practical danger. Cases of this accident have been recorded by Ribemont-Dessaignes and Grosse, by Malherbe and Grosse, and by Ivanoff.

Miscarriage Risks.

The next series of risks to be considered are those associated with miscarriage. The actual frequency of miscarriage where fibroids are present has been a matter of some difference of opinion. According to Kelly, 25 per cent. of the patients abort. Pinard put the figure at 21 per cent. As the average for all women is stated to vary between 20 and 25 per cent., any increased frequency due to the presence of fibroids can only be very trifling. This opinion is also held by Herman. Presuming next that a patient with fibroids does miscarry, does the presence of the fibroid materially increase the risk of this accident? Cases have been recorded where portions of placental tissue have been retained *in utero*, and extreme difficulty has been experienced in evacuating the uterus owing to the fibroid blocking the lower part of the canal. As a result severe sapraemic symptoms have supervened. Although one is bound to admit that this difficulty with its unfavourable results may occur in exceptional cases, it need not and ought not to be regarded as anything more than an exceedingly rare complication. Should it be found impossible to evacuate the uterus of retained products, and sapraemic symptoms persist, the timely removal of the uterus by the abdominal route will almost certainly cure the patient if the operation is not postponed too long.

Labour and Puerperium Risks.

The third series of risks are those associated with labour and the puerperium. Where the fibroid is situated in the pelvis, below the presenting part, it is bound to cause obstruction to delivery, with the probable sequence of rupture of the uterus, if suitable treatment is not undertaken. Cases of this kind, however, are most likely to occur where the existence of the fibroid or fibroids is unknown to the patient or her medical attendant before the onset of labour. The dangerous complications which may ensue from obstructed labour might be entirely avoided by an examination during pregnancy. Such an investigation would give timely warning of the difficulties which must inevitably occur, and would prevent one from being suddenly confronted with a case of this kind at a stage of labour too far advanced for successful treatment. Elbrecht holds the view that rupture of the uterus is a serious risk in cases of interstitial fibroids, owing to thinning of the uterine wall over the tumour, quite apart from the question of the fibroid causing any obstruction to delivery. I think the majority of obstetricians would consider this a more grave view than is warranted by practical experience. The risk of severe *post-partum* haemorrhage where fibroids complicate labour has been a cause of grave anxiety to medical practitioners for a long time, and the balance of opinion seems to show that there is some increase in the frequency of this complication. There is, however, no need for any serious alarm, but special care should be exercised in the management of the third stage of labour.

During the puerperium a fibroid tumour may become extruded and present in the vagina. If this happens it will certainly slough and sapraemic symptoms will follow. An interesting case of this kind was seen by me in October, 1909.

The patient, a primipara of 37, had a normal though prolonged confinement, which had to be assisted by forceps. During the first two weeks of the puerperium the temperature ranged between 99° and 100.5°, the lochial discharge being free all the time. When examined by her doctor a tumour could be felt projecting down into the cervical canal. A few days later the temperature fell to normal but the vaginal discharge became offensive, and, when seen by me, a large sloughing mass as large as the closed fist was found in the vagina. The whole mass was removed with scissors under an anaesthetic, and the patient made an uninterrupted recovery.

Necrobiotic change occurring in fibroids after delivery is not usually a cause of urgent symptoms, but occasionally the pain and tenderness over the tumour with elevation of temperature may suggest pelvic inflammation. In a case under my care at St. Thomas's Hospital, where the three symptoms mentioned were unusually marked, the temperature ranging between 100° and 103°, the necrotic fibroid was situated between the layers of the right broad ligament, but there was not a sign of any surrounding peritonitis. Clinically the case suggested an inflamed, and possibly suppurating, dermoid cyst of the ovary. True suppuration of a fibroid tumour is an extremely rare complication. It is invariably due to some failure in the antiseptic technique, and is, therefore, an avoidable complication.

I have seen one case of true suppuration of a fibroid following delivery, in a patient aged 37.

She was delivered of a stillborn child in March, 1906, following which a parametritic abscess formed which burst into the vagina. A fibroid tumour was discovered at this time. The patient continued to have a hectic temperature and steadily lost flesh. She had a continuous vaginal discharge of blood, and occasionally clots. When seen in September the tumour had increased very greatly in size, and was then as large as a six months pregnancy. It was very tender on pressure and quite elastic. Abdominal hysterectomy was performed by me early in October, and on incising the tumour after removal it contained 3½ pints of foul-smelling pus, with a large slough from

the fibroid in the middle of the abscess cavity. The temperature, which had varied between 100° and 102° and 103° for months, fell to normal on the eighth day, and the patient made a good recovery.

I have endeavoured to give a fair and I hope unprejudiced criticism of the several dangers to which women are exposed where pregnancy and labour are associated with fibroids. A thorough and correct appreciation of these can alone form a reliable basis for successful treatment. It will be evident from what has been already said that in my opinion only a very small number of cases call for any interference whatever during pregnancy. This is the view held by Murray and Donald, who maintain that in the majority of cases pregnancy and labour are not seriously influenced. The grave warning words which have been expressed by some writers on this subject are contrary to experience, and would not, I believe, be accepted by practical obstetricians. The weighty words of Hofmeier may with advantage be repeated at the present day. Hofmeier says:

> I believe most positively from my experience that the complications which myomata present during pregnancy, birth, and the puerperal state, cause actual earnest dangers in only a few cases, and may be quite essentially diminished by patience, a cautious treatment of the birth, especially by its strict antiseptic conduct, and by careful attention to the puerperal period.

At the International Medical Congress at Budapest held last year, this subject was considered very fully. Schauta of Vienna considered that in the majority of cases fibroids caused no complications during labour, and therefore he was of opinion that active interference during pregnancy is generally unnecessary. Engström dwelt on the reduction in the size of fibroids which may occur during the puerperium, but considered that they may take on fresh growth after involution is complete. Pozzi found that some of these tumours increased in size during pregnancy, but rarely to such an extent as to endanger life. He considered the increase due either to oedema or to increase in the actual elements of the tumour. Pozzi thought that in the majority of cases no special supervention was necessary.

In considering the treatment of these cases, it will be convenient to divide them into three groups:

Group A.—Cases in which the fibroid tumour is situated in the pelvis and *must necessarily* obstruct delivery unless the condition is relieved. These cases will include fibromata of the cervix.

Group B.—Cases in which the fibroids arise from or encroach upon the lower uterine segment, and may *possibly* cause obstruction to delivery.

Group C.—Cases in which the fibroid or fibroids are entirely abdominal tumours, and will *certainly not* interfere with delivery *per vias naturales*.

In the first group, where the fibroid is growing from the posterior wall of the uterus and occupies the sacral hollow, the symptoms will be slight or severe according to the extent to which the tumour fills the pelvis, and the presence or absence of degenerative change. Two methods of treatment are available. If there is some mobility of the tumour, it is usually possible under anaesthesia to push the tumour up above the pelvic brim. When this has been done the further course of the pregnancy is usually uneventful. If the tumour is firmly impacted in the pelvis, and great force would be required even under an anaesthetic to push the tumour up out of the pelvis, it is safer to perform abdominal section with a view to enucleation of the tumour from the uterine wall. It has been pointed out by John Phillips that there is considerable risk of tearing the peritoneum over the tumour and lacerating small vessels where forcible attempts are made to push these tumours out of the pelvis. In the case of small fibroids of the cervix, when discovered during pregnancy, it may be possible to enucleate these tumours *per vaginam* without interfering with the course of pregnancy. Olshausen is of opinion that the removal of polypi, and especially the enucleation of submucous fibroids from the cervix, should be postponed till the end of pregnancy, as labour may be expected to follow closely upon such operative interference. Where the tumour is of larger size, and not causing any definite pressure symptoms, it is certainly best to postpone interference till the time of labour, when Caesarean section followed by enucleation of the tumour, or complete hysterectomy, may be performed. The enucleation of large cervical fibroids *per vaginam* during pregnancy or labour is not to be recommended, owing to the risk of severe haemorrhage, which may be difficult to control. Where the interference becomes necessary during pregnancy, and the cervical fibroid is of large size, filling up the pelvis, the pregnancy will probably have to be sacrificed, and total hysterectomy will be necessary. In actual practice these cases are very rarely met with, as the patient is usually sterile where large cervical fibroids are present.

The second important group of cases are those in which the fibroids are often multiple, and one or more of them are found encroaching on the lower uterine segment. In this class of case some anxiety must be felt as to whether the tumours, by reason of their position, will cause any obstruction to delivery. The fact, however, that we cannot predict with certainty the behaviour of these tumours during labour is no justification for advising surgical interference during pregnancy in the absence of urgent symptoms. Experience shows that an expectant attitude is followed by the most encouraging results to both mother and child. It not uncommonly happens that fibroids materially alter their position during labour in such a way that the tumours gradually recede from the dangerous zone. The presence of a fibroid encroaching on the lower uterine segment does not necessarily cause disaster. It is our duty in these cases to allay unnecessary fears, and not to excite dread and anxiety. My late colleague, Cullingworth, recorded four interesting cases in support of the expectant policy during pregnancy. In the first case the patient, who had a fibroid situated low down and behind the uterus, was advised to have abortion induced. Cullingworth decided against any interference and advised the patient to wait events, with the satisfactory result that she had an uncomplicated labour, and moreover had three subsequent children at term. In the second case, when arrangements had been made for the performance of Caesarean section at the thirty-ninth week for an obstructing fibroid, Dr. Cullingworth predicted that the tumour would probably recede during delivery. This prediction proved to be correct, the child being delivered at full term, with most favourable results to both mother and child. The third and fourth cases were of a similar nature and had equally satisfactory results.

The third group consists of those cases in which the fibroids are entirely abdominal tumours. These include cases of subperitoneal pedunculated fibroids and also interstitial fibroids. In the vast majority of these cases the absence of severe symptoms makes operative interference unwarrantable. Cases in which the tumour is subperitoneal may complain of constant pain or severe discomfort. The greater number of these lend themselves readily to the operation of myomectomy, which is equally available in cases where the tumour is pedunculated and where it is sessile. When the subserous growths are of small size, and causing no symptoms, their removal is unnecessary. Condamin has reported four cases of myomectomy during pregnancy and concludes that severe and lasting pain may demand this operation. He further adds that if the presence of a fibroma makes normal labour impossible, operation should be undertaken as soon as possible to prevent abortion and to remove the obstacle to delivery. If a fibroma has already caused abortion more than once, operation in his opinion is undoubtedly justifiable.

The cases in which interstitial fibroids justify interference during pregnancy are few and far between. These cases must be very carefully investigated, and one must recognize the fact that where operative treatment is undertaken in these, it will very probably be necessary for the whole uterus to be removed and the pregnancy sacrificed. Mere prophylaxis, that is, operating for dangers which may arise, where there are no urgent symptoms, has, as Howard Kelly says, no field here. The mere fact that a patient suffers from a certain amount of inconvenience or increased discomfort, without any serious effect on the cardiac or respiratory organs, is not a sufficient reason for sacrificing the pregnancy. Hence the importance of postponing operative interference in all cases where hysterectomy may be necessary till full term or at least till the child is viable. In all these cases, if it should seem possible to enucleate the tumour when the abdomen is opened, the operation of myomectomy is still to be preferred. There are, however, a certain number of cases

where the tumour is of large size or where the fibroids are multiple and have shown marked increase during pregnancy, in which operative interference is not only right, but imperative, and in which abdominal hysterectomy is the only suitable operation to undertake.

The induction of abortion has been almost universally discountenanced as a method of treatment in these cases. The pregnancy is sacrificed, and the patient is not relieved of her tumour. In this connexion one may mention an interesting case, reported by Webster in 1905.

r The patient, aged 31, had a history of twelve days' haemcr-chage. On examination a fibroid of the cervix was found complicating a four and a half months pregnancy. As Webster onsidered the uterus ought to be emptied, and the necessary manipulation could not be safely undertaken *per vaginam*, the abdomen was opened, and the fibroid enucleated by an incision low down on the posterior surface of the uterus. An incision was then made through the raw surface then exposed, and the uterine contents evacuated. The raw surface was subsequently brought together with sutures, and finally the peritoneum closed over these.

The induction of premature labour in the case of fibroids causing, or likely to cause, some obstruction, is equally to be condemned, as a difficult delivery with a premature child is almost certain to end in the child being stillborn, which constitutes an obstetrical failure.

Having strongly advocated the operation of myomectomy in all cases in which it is feasible as the operation of selection where surgical treatment has become necessary during pregnancy, the next question is whether myomectomy should be done for an obstructing fibroid, or for one causing severe pain and probably necrotic, where other fibroids of moderate size are present in the wall of the uterus, but not in themselves causing any symptoms or likely to interfere with delivery. In my opinion the answer to this question should be in the affirmative. There is in such cases no certainty that the tumours left in the uterus will be a cause of further trouble after delivery, and it is known that occasionally fibroids may disappear during involution. Cases have been recorded—one by Amand Routh—where a myomectomy had been done during pregnancy, and at the time of labour a second tumour was discovered in the lower segment of the uterus, obstructing delivery and rendering necessary further surgical interference. In every conservative operation there must be a little uncertainty as to the future. If we are going to determine our surgical procedures on such lines that all possible risks of further trouble is to be excluded, conservative operations would have to be abandoned. Every ovariotomy would have to be made double, and certainly myomectomy could under no circumstances be advised, for fear of embryo fibroids developing later and necessitating the more radical operation. The employment of such radical measures as those indicated would, of course, be denounced by us all.

In advising myomectomy during pregnancy we have more than the mere conservative operation on the uterus to justify our selection. The pregnancy which is under consideration may be the woman's only chance of bearing a child. With such a possibility in one's mind it is our duty to exercise the same jealous care in interfering with the course of pregnancy as is generally the practice in the case of pregnancy uncomplicated by fibroids, in which cases the induction of abortion is not regarded as a justifiable operation, unless the patient's life is endangered by the continuance of the pregnancy. In the discussion of a paper by Donald, read before the Obstetrical Society in 1901, Bland-Sutton referred to the case of a patient who had aborted on three occasions, and in her fourth pregnancy was found to have a fibroid impacted in the pelvis. Myomectomy was performed by him, and the patient went on to term. When the patient was delivered the uterus was found to be tuberose with fibroids, and when seen by Bland-Sutton ten days later he found this unfortunately the case. Bland-Sutton cited this case as a failure for myomectomy. I, however, should congratulate Bland-Sutton on his good judgement in selecting the conservative operation and preserving the life of the child. The patient, after having had three fruitless pregnancies, succeeeded in carrying a child to term. Whether the tuberose uterus discovered after labour was the cause of any symptoms requiring further surgical treatment was not stated. In the interest of the child, quite apart from the advantage of a conservative operation on the mother, the operation of myomectomy should always be selected where this operation is possible, even though there may be in a certain number of cases a possibility—and it is not more than a possibility—that further surgical treatment may at some future time become necessary.

If any are of opinion that this is too great a risk to subject a woman to for the sake of the child, consider for one moment the change of practice that has taken place during the last decade in the matter of sterilizing the patient when performing Caesarean section in cases of contracted pelvis. At the present time the majority of obstetricians not only do not recommend it, but are of opinion that it is unjustifiable to sterilize the patient as a safeguard against future pregnancy. During the last seven years the old practice of sterilizing the patient has been entirely abandoned at Queen Charlotte's Lying-in Hospital. That is to say, we deliberately refrain from adding a simple procedure to the operation of Caesarean section, which would save the patient from the inevitable risk associated with a further pregnancy, in cases where we know that delivery of a living child by the natural passages is impossible. This change of practice, which is done entirely with a view to future child-bearing, has become justifiable in consequence of the greatly diminished mortality in abdominal surgery in recent years. If, then, it is right to leave a woman with the inevitable risk which a second Caesarean section must involve, surely the conservative operation of myomectomy during pregnancy is more than warranted in the interest of the child, in cases where it can be performed, even though other fibroids are present in the wall of the uterus, which though causing no symptoms at the time, may conceivably necessitate a more radical operation at some future date.

While we must always hold it to be our paramount duty in every case of pregnancy to ensure the safety of the mother, there are special circumstances in the case of fibroids complicating pregnancy which compel us to regard with almost the same consideration the preservation of the child. We ought thoroughly to appreciate the responsibility which we take when the course of treatment decided upon involves the sacrifice of the pregnancy. When one remembers the serious conditions to which a patient is often reduced before it is considered advisable to interfere with a normal pregnancy, in such cases as albuminuria, cases of cardiac disease, hyperemesis gravidarum, and other grave conditions, it can hardly be maintained that in the majority of cases the patient with a fibroid tumour complicating pregnancy is allowed to be exposed to nearly so great a risk to her life or to her future health before interference with the pregnancy is recommended.

One ought never to forget that the occurrence of a further pregnancy in any woman suffering from fibroids must be a doubtful matter, and the only chances of motherhood may depend on the patience and good judgement exercised by her medical attendant in the management of the existing pregnancy. Although it has been necessary to refer to the treatment of fibroids during labour on several occasions in the course of this paper, I have not attempted to discuss it at length, as it seemed to me that the discussion would lose in value if it extended over too wide a range. Moreover, the treatment of fibroids during labour is a very important subject, and might well be considered on some future occasion by itself.

DISCUSSION.

Dr. Amand Routh (London) was in almost entire agreement with the views expressed. If any operative treatment was required during pregnancy it should take the form of a conservative myomectomy, and the pregnancy be allowed to proceed to full term. He had never removed a uterus during pregnancy for this complication. Torsion of the pedicle leading to acute changes of the fibroid could be treated by myomectomy with very little risk of premature labour being induced. Imbedded fibroids undergoing necrobiosis or red degeneration could be enucleated, but the risk of premature labour was greater. Dr. Tate had raised a very important question as regards the correct treatment when myomectomy had been found to be necessary during pregnancy or labour, and other fibroids were found to be present. If the child was not viable, myomectomy of the degenerating fibroids should alone be done, and the case

allowed to go to full term. For this was probably the only chance of the woman bearing a child. If, however, the woman was at or near full term, and the child likely to recover if Caesarean section were performed, the uterus should be removed at once if the circumstances—that is, competent skill of the operator and good condition of the patient—permitted. It was hardly fair to compare hysterectomy in these cases with sterilization after Caesarean section for contracted pelvis, for the fibroids were not fixed, unalterable structures, but were liable to undergo degenerative changes, to cause severe pressure symptoms, and another pregnancy was very unlikely to occur. In such cases hysterectomy was advisable. He hoped that this discussion would lead to the absolute discontinuance of hysterectomy before fetal viability, and that every chance would in future be given for the pelvic fibroid to rise up out of the pelvis. The only fibroids which did not rise up out of the way were cervical, some intraligamentous fibroids, and fibroids adherent to the pelvic floor.

Dr. HERBERT SPENCER (London) agreed generally with the treatment of fibroids complicating pregnancy as presented by Dr. Tate. In the speaker's experience, the complication of pregnancy with fibroids was fairly common; he had four such cases under his care at the present time. In twenty-three years he had seen a large number of such cases; a considerable proportion of all primiparae of 35 years of age or more would be found to have one or more fibroids in the uterus, if carefully examined, especially after labour. In the short time at his disposal he would not enter into details, but would give a general view of his experience, which he thought supported Dr. Tate's views. He (the speaker) had only once performed myomectomy during pregnancy. It was on account of a tumour in the broad ligament, the solid portion of which weighed nearly 18 lb. The operation was done at term six hours before delivery. The patient recovered well. The speaker had only three times found it necessary to perform Caesarean section for uterine fibroids, and once for ovarian fibroid mistaken by Sir John Williams and himself for uterine fibroids. All four women recovered. Dr. Tate had not called attention to inversion of the uterus produced by submucous tumours which caused abortion. The speaker had seen two such cases, and had enucleated the tumour and replaced the fundus, and the patients, who had fetid discharge, recovered well, and one had had children since. He had had a case with a temperature of 105° very like Dr. Tate's, and he had treated it similarly by enucleation with success. Too often in these cases of septic submucous fibroids it was considered to be necessary to remove the uterus. He had never found it necessary to do it. Of the large number he had seen he had only known two patients die from fibroids complicating pregnancy, labour, or the puerperium. One of these had septic peritonitis from torsion of the pedicle of a subperitoneal tumour. Though operated on immediately, death followed. The other died from intestinal obstruction caused by the gripping of intestine between two subperitoneal tumours.

Mr. ALBAN DORAN (London) said that Dr. Tate had shown that the question was to be settled by common-sense surgical and obstetrical principles. Hysterectomy for fibroid in pregnancy might seem difficult and dangerous to a surgeon not used to operate on the gravid uterus, on account of the formidable array of big blood vessels running over the tumour; but to those accustomed to abdominal sections on female subjects, those who expected to meet with huge engorged veins, the operation was perhaps too easy because of the hyperplasia of the pelvic connective tissue, which made the dissection of peritoneal flaps easy, far easier than in a subject not pregnant. It was clear that when the fibroid was in or near the fundus operation was rarely called for. When near or in the lower segment below the fetus delivery without danger was, as Dr. Herbert Spencer had shown, quite frequent. But when the uterus was riddled with fibromyomas of all types—submucous, interstitial, and subserous—it was a spoilt organ, a source of immediate and future danger to the patient, and therefore, as a rule, such a uterus should be removed in early pregnancy. Mr. Doran turned attention to a specimen of gravid uterus of this type on view in the museum, and belonging to the College of Surgeons. There was a pedunculated subserous fibroid, and if that had been the sole growth in this case, hysterectomy would have been absolutely unjustifiable. But there was also a big growth beneath the fetus, burrowing in the left broad ligament; the uterine wall was riddled with small interstitial fibromyomata; and, above all, the fetus was ill formed, and there was placenta praevia. Mr. Doran, after consultation with Dr. Hubert Roberts, had removed the uterus, and thus saved the patient from much risk. In the other cases where Mr. Doran had operated there were similar complications. Lastly, he insisted that the ovaries should always be saved if possible, and that the cervix should not be sacrificed save in exceptional cases.

Mr. CHRISTOPHER MARTIN (Birmingham) said that severe uncontrollable vomiting may be caused by a small interstitial fibroid complicating pregnancy. In two such cases he had had to induce abortion to save the mother's life. Acute red degeneration of a fibroid might come on during pregnancy and cause symptoms which urgently called for surgical interference. In one case the patient was a young woman recently married, and was three months pregnant. She suddenly became very ill. She had acute abdominal pain, vomiting, quick pulse, fever, and a rapid increase of the tumour. The symptoms, in fact, resembled those of a strangulated ovarian cyst complicating pregnancy. He opened the abdomen, and found the tumour was a fibroid which was undergoing acute red degeneration. He performed myomectomy, and preserved the uterus. The patient aborted on the second day, but otherwise she made a good recovery. A year later she became pregnant, went to full time, and had a living child without any difficulty. Myomectomy during pregnancy was often a more difficult and risky proceeding than hysterectomy; but in most cases it was well worth the risk, in order to give the patient the chance of a living child. Where the fibroid was impacted in the pelvis it might greatly impede labour and call for Caesarean section. Even if the child were delivered *per vias naturales*, the tumour might be so bruised and crushed that it underwent necrosis. Only recently he had opened the abdomen and removed a necrotic fibroid during the puerperium. The woman had been previously delivered by craniotomy at full term on account of a fibroid blocking the pelvis. After delivery she became acutely septic, and developed peritonitis. When he opened her abdomen the fibroid was found to have undergone complete necrosis. After it was removed all her grave symptoms subsided, and she got quite well. He quite agreed with Dr. Tate's views on the conduct of pregnancy complicated with fibroids. He thought that when surgical interference was necessary myomectomy should be the operation of choice, and hysterectomy only done when the former operation was not feasible.

Dr. ARTHUR J. WALLACE (Liverpool) joined in the discussion merely to mention a case and show the specimen that illustrated the tolerance of the pregnant uterus to operative interference. A patient of 45, four months advanced in her only pregnancy, suddenly suffered from a peritoneal crisis and became very ill. On a diagnosis of probable torsion of the pedicle of an ovarian tumour, the abdomen was opened, and a large oedematous fibroid (14 in. × 10 in. × 6 in.) found to be sessile on the fundus uteri. Its attachment was extensive (8 in. × 6 in.) and so deep that a circular patch of decidual tissue was exposed during the enucleation. Recovery ensued, followed five months later by delivery of a healthy child at term. Despite the extensive wounding of the uterus there was never the slightest indication of any tendency on the part of the organ to empty itself.

Dr. J. NIGEL STARK (Glasgow) said the question of treatment depended on the position of the tumour and also upon the size. This latter point was of importance; when the tumour was very large and interstitial there might be good reasons for inducing labour first of all and then in doing myomectomy afterwards. One of his patients, a woman aged 38, who had a very large interstitial tumour near the fundus, and was six months pregnant, was breathless and very ill, and myomectomy was performed.

The haemorrhage was most alarming, and after all abortion occurred in a few days. Probably it would have been better to have induced labour first of all and then performed myomectomy.

Dr. HUBERT ROBERTS (London) expressed himself as in entire sympathy with Dr. Tate's views as to the small proportion of cases of pregnancy complicated by fibroids that required interference. The latest statistics of Pinard showed that fibroids complicated labour in 84 out of 13,915 consecutive cases of labour = 0.6 per cent. Champneys, Spencer, Herman, Routh, Olshausen, and others had shown that interference was rarely necessary, and that merely because fibroids *were present* this in itself did not indicate operation. The whole question resolved itself into two main points: (*a*) What is the effect of fibroids on pregnancy and labour and the puerperium? (*b*) What is the effect of pregnancy, labour, and the puerperium on fibroids? Dr. Tate had pointed out the rarity of such complications as torsion of the pedicle, pressure symptoms, and degeneration. As regards obstruction to labour, Dr. Roberts agreed that this was uncommon, and that at Queen Charlotte's Lying-in Hospital operation was rarely needed, though plenty of women were delivered naturally who had fibroids. Munro Kerr's and Spencer's statistics showed that in two series of 35,000 cases of labour abdominal section was only required four times. The speaker pointed out that fibromata of the ovary might also complicate delivery. A fatal case of this nature was recorded by Griffith, the specimen being in St. Bartholomew's Hospital Museum. As to malpresentation, inertia, and the risks of *post-partum* haemorrhage, it was not quite certain that these were commoner with fibroids than in ordinary labour. The effects of pregnancy on fibroids were important, and had been fully discussed by Dr. Tate. The speaker did not think that the red degeneration described by Fairbairn and Bland-Sutton was a very serious matter, but that septic infection after labour of a fibroid uterus might lead to very grave results. Some of the most dangerous cases were those in which labour was obstructed by cervical fibroids or fibroids of the broad ligament, as described by Doran. As regards treatment, abortion and premature labour were not to be advised, as the child must be considered. If obstructed labour occurred, they had to choose between myomectomy and hysterectomy. The hysterectomy might be partial or complete. There were grave risks in attempting to deliver a child by forceps or craniotomy when a fibroid obstructed delivery, and should septic infection occur the best course seemed to be removal of the whole uterus. This should also be done in cases of Caesarean section where repeated attempts had previously been made to deliver the patient per vaginam.

Dr. ARNOLD W. W. LEA (Manchester) pointed out the gravity of fibromyomata complicating pregnancy and the puerperium. The mortality had no doubt been diminished by recent improvements in technique, but danger might arise at any moment in the course of pregnancy or after delivery. In a series of 8 cases which he had arranged in tabular form, operation was required in 5 during pregnancy and in 3 during the puerperium. In only one case was myomectomy performed; in all the others hysterectomy was necessary. In the 7 instances the tumours removed showed degenerative or inflammatory changes: Myxomatous degeneration with cysts, 1 case; red or carneous degeneration, 4 cases; suppuration, 2 cases. The opportunity for making bacteriological investigations occurred in 4 instances: In 2, examples of red degeneration during pregnancy, cultures from the interior of the tumour were found to be sterile; in another case of extirpation after delivery diplococci were found on microscopic section; in a suppurating subperitonea myoma removed by vaginal hysterectomy, streptococci and colon bacilli were found in the tumour. A decision could often only be made after opening the abdomen. In cases of infection of all tumours after delivery there could be no doubt that abdominal hysterectomy was the only effectual method of treatment. All the cases made good recoveries. With regard to the mode of operation during pregnancy, the choice lay between myomectomy and hysterectomy. Myomectomy was suitable if the tumour was pedunculated; but all statistics showed that in tumours impacted in the pelvis, involving the cervix or invading the broad ligament, myomectomy was a much more dangerous procedure than complete hysterectomy.

Dr. ARTHUR GILES (London) said that the tone of the speakers struck him as one of undue optimism, and he was glad to hear Dr. Lea sound a somewhat different note. The dangers of fibroids complicating pregnancy were no doubt relatively slight when patients were under the care of expert obstetricians and gynaecologists; but it was necessary to consider the matter on a broader basis than this, and discuss the risk of patients under ordinary conditions. He ventured to say that of all cases of pregnancy associated with fibroids, at least 50 per cent. would be found to present some complication. The way they should regard the question was this: If a woman were known to have a fibroid, would they advise her to marry and run the risk of pregnancy? Or, if a patient were known to have a fibroid when pregnant, would they sanction her going into a remote country district beyond the reach of expert assistance? He felt sure that the combination of the two conditions was one involving very serious risk, and that the only chance of safety lay in the patient being kept under close observation, so that interference could be resorted to at any time if dangerous symptoms should arise. He shared the view of those who deprecated the induction of abortion or premature labour; he would advocate that a watchful attitude should be assumed if there appeared to be a chance of pregnancy going to full time; that if acute symptoms showed themselves during pregnancy a conservative myomectomy should be resorted to if possible; and that, if interference were required during labour, a Caesarean hysterectomy should be carried out. If the abdomen had to be opened on account of obstructed labour, he thought it a pity to leave a uterus with fibroids, exposing the patient to all the dangers of a repetition of pregnancy with fibroids.

Dr. LLOYD ROBERTS (Liverpool) said myomata were not common in pregnancy. Often they were not detected during pregnancy, and occasionally not even during labour. As myomata occurred about the age of 40, the woman was generally sterile. Pregnancy with myoma was often not dangerous, but this was not invariably the case. The myoma might prevent the development of the uterine muscle, or cause haemorrhage, abortion, premature labour, and localized peritonitis from impaction. This latter trouble often rectified itself as pregnancy advanced; the uterus and the tumour was crowded out of the pelvis out of harm's way into the abdominal cavity. If the tumour did not rise with the uterus out of the pelvis it might be lifted out. Myomectomy was the ideal treatment, by abdominal section if the tumour were situated in the upper segment of the uterus, and by the vaginal route if the growth was attached to the lower segment of the womb. He would not interrupt the progress of the pregnancy by inducing abortion or premature labour, as there was more risk of septic infection by this method of treatment than any other. The last thing he would do was the removal of the uterus. These tumours often became lessened, and sometimes disappeared altogether after labour. Removal of the uterus took away the chance of this hoped-for result and also the possibility of a future pregnancy. If these growths showed signs of necrosis, and there be septic infection, he would advise operative measures at once. As to sterilization, he would not in these days sterilize the patient.

Dr. W. C. SWAYNE (Bristol) said it was obvious that a large proportion of cases in which fibroids complicated pregnancy did not need interference, but occasionally symptoms were produced which demanded operative interference. The following cases were instances in point:

In a patient, a primipara aged 33, four and a half months pregnant, a large fibroid filled the entire upper portion of the abdomen; pain, dyspnoea, rapid pulse apparently rendered the continuance of the pregnancy likely to be fatal. A large pedunculated fibroid was removed from the fundus uteri. The patient made a good recovery, and was safely advanced in pregnancy three months later, after which she was lost sight of.

In the next case, a primipara aged 27, pregnant five months, acute abdominal symptoms occurred, and on opening the abdomen axial rotation of the uterus was found under a large fibroid attached to the right cornu lying in the left iliac fossa. This

was enucleated, and its bed sewn up with two layers of catgut sutures. The patient made a good recovery, but her pregnancy was interrupted one month after the operation.

In a third case, a primipara aged 23, pregnant about two and a half months, suffering from dysuria and pain in the pelvis, was found to have a fibroid growing from the supravaginal portion of the cervix the size of the fist. As while she was under observation the fibroid rapidly increased in size, and the symptoms became more severe, the tumour was enucleated after opening the posterior cul-de-sac pouch of Douglas. The patient went to term, and was delivered without any complication of a living female child.

The question of operative treatment must be decided on the conditions present in the individual case, and the question of the exact method of dealing with the fibroid must be decided after laparotomy had given access to the tumour itself. He had a previous similar difficulty to that described by Dr. Lockyer on the removal of the placenta in a case of fibroid.

Mr. J. Furneaux Jordan (Birmingham) said that it had fallen to his lot to have to deal with the following cases: A woman six months pregnant with a very large fibroid, weighing over 8 lb. after removal; a case of labour obstructed by a pedunculated subperitoneal fibroid which filled up the pelvis; a case of inversion of the uterus after labour due to a fibroid in the fundus; and a case of a large sloughing submucous fibroid causing profuse, almost fatal, haemorrhage after labour. In all of these the treatment adopted was myomectomy, and in each case a good result followed. In the first two cases there had been a subsequent pregnancy. On this experience he would support his colleague, Mr. Martin, in his advocacy of myomectomy. He would also remind Dr. Giles that the expert gynaecologist was everywhere or almost everywhere; that they did not minimize the gravity of these cases, but realized as the years went by that they were better and better able to treat them safely and satisfactorily without having to resort to hysterectomy.

Dr. Mary Scharlieb (President) cordially agreed with Dr. Tate. As a rule, fibroids caused no great difficulty during pregnancy, parturition, or puerperium, but each case must be judged on its own merits. A cervical or intraligamentous growth which would cause obstruction to labour would call for operation, whereas a tumour in the upper part of the uterus would not cause obstruction, and did not offer an indication for operation, unless by its size, or by reason of some accident (such as torsion or degeneration), it was a source of danger. The operation of election was certainly myomectomy, which ought to be preferred to hysterectomy, unless the uterus were infected or so riddled with fibroids as to be a useless organ and a source of danger. She had performed myomectomy on several pregnant women, sometimes for symptoms due to degeneration of the growth, and once for a fibroid which coexisted with an ovarian dermoid and pregnancy. The two growths were removed. The patient was safely delivered at term of a living child.

Dr. Tate, in reply, said he was gratified to find that he had the support of nearly all the speakers in the remarks he had made in opening the discussion. In reply to Dr. Spencer, he thought that in a few cases the pain caused by fibroids was sometimes so severe that operative interference became necessary. Dr. Tate did not agree with Dr. Martin that the fibroid in his case was the cause of the uncontrollable vomiting owing to the tumour interfering with the expansion of the uterus. If this were so, why did not the tumour cause vomiting in the subsequent pregnancy?

THE ORIGIN AND PREVENTION OF PUERPERAL FEVER.

By Professor D. Döderlein,
Munich.

Of the large number of the factors in puerperal infection two stand out with ever greater clearness: these are the danger of (1) infection from within the female generative organs themselves, and (2) inoculation by pathogenic organisms from without during labour or the puerperium.

As for the former, the flora of the vagina afford extensive protection against virulent infection, at least during pregnancy, during normal labour, and also during the early and most dangerous period of the puerperium. Only one definite micro-organism has its natural habitat here—the *Bacillus vaginae*, which was first described by me, and which plays the important rôle of preventing the development of other micro-organisms, especially those of a pathogenic kind, by the production of lactic acid. This circumstance must be our guide in treatment. If we find abnormal conditions of the flora in the vagina during pregnancy we should try to favour the propagation of these normal inhabitants of the vagina.

Measures for the disinfection of the internal organs of generation at the time of birth are, however, much more important than the prophylactic measures to be taken during pregnancy. On this very contradictory views are held. While on the one hand some aim at avoiding danger of infection by special measures of disinfection—irrigation and rubbing the vagina with disinfecting substances—others maintain that such measures are of no advantage, and that Nature has surrounded the physiological act of birth with all necessary protection. In favour of this latter opinion—which I also hold—are the results of the bacteriological examinations of the secretion of the female genitalia, which show that the antagonistic relations of the bacteria to each other afford the simplest and best means of combating this autochthonic danger of infection.

Practical experience also favours this natural method of treatment of normal parturition, though statistical evidence on the point must be examined with care if false conclusions are not to be drawn.

The comparison of clinics employing fundamentally different methods of treatment may easily lead us astray. Different results do not authorize the conclusion that the cause for these differences lies in special principles of treatment. Conditions in different hospitals vary too much for general conclusions to be formed from single measures.

Whether prophylactic treatment of the genitalia of the parturient woman is advantageous, or, on the contrary, even injurious, can only be decided by comparing the results of different methods at one and the same hospital and during the same period.

I have at different times made such investigations in my clinics at Tübingen and Munich. Alternately one parturient was disinfected and the other not, so that there were always at the same time lying-in women of both classes together in the hospital who had been confined at the same time, and in other respects than that of vaginal disinfection under exactly the same conditions. The results in the Tübingen clinic, published by Baisch,[1] covered 500 births, during which the parturient women were irrigated with 1 litre of sublimate solution, 1 in 1,000. During the irrigation the walls of the vagina were rubbed with two fingers of a gloved hand carefully and cautiously, but still as thoroughly as possible. As all internal examinations of these women were made exclusively with a gloved hand, and the external genitalia were previously disinfected, the introduction of microbes from without was excluded.

In the same period there were 500 lying-in women not subjected to vaginal disinfection. Of the 500 disinfected women, 12 8 per cent. had fever, while of the 500 not disinfected, only 8 per cent. had fever. After deducting those who had fever not originating from the genitalia, there remained 5.2 per cent. with fever among the non-disinfected and 10 per cent. among the disinfected.

The investigations recently made in the Munich Lying-in Hospital in the same way, and published by Eisenreich,[2] furnished similar results. We used here an irrigation of ½ to 1 per cent. solution of lactic acid. The results showed that, of 460 parturient women disinfected therewith, 6.29 had fever, while of 477 not irrigated, only 3.5 per cent. had fever.

Thus, in both series of observations the disinfected women were twice as badly off as those who were not disinfected. These results force us to the conclusion that the disinfection of the internal organs, by whatever means carried out, is not only of no use, but rather causes injury.

If we had not the theoretical bacteriological investigations of the flora of the female genitalia, these clinical results would be extremely puzzling, as being opposed to all our fundamental views of antisepsis. But, as things are, they are readily explicable.

The protective action of the natural flora of the vagina is disturbed by the disinfection. Moreover, as the result of the energetic and repeated disinfection with watery solutions, the secretions of the genitalia are removed, the vagina becomes dry and its epithelium swollen. The probability of the mucous membrane being abraded during birth, and thereby more exposed to invasion of bacteria, is consequently increased. But however matters be explained, the fact remains that when the flora are disturbed by disinfective measures, the chances of parturition remaining entirely physiological are decreased. As for infection from without, the irrigation tubes or other instruments introduced into the genitalia during labour may contain microbes which have remained on them from former use. If such instruments have been recently used on infected cases, and have not since been sterilized, the danger is obvious, but it is so easy to prevent this that the risk only need be mentioned.

It is much more difficult to prevent the danger from the examining hand. This danger was discovered and proclaimed by Semmelweis, and though his views were at first disputed, it is now fully established that he was right; in other words, the examining hand is the principal source of infection of parturient women, and serious cases of puerperal fever are caused almost entirely in this way.

There are two ways available of avoiding this danger—either to give up the internal examination of the parturient woman entirely, or so to prepare the examining hand that it can carry no poison into the genitalia. It has long ago been shown by numerous clinical observations what an advantage lying-in women have when their internal examination can be and is avoided.

By developing methods of external examination it might be possible to render the establishment of a complete diagnosis so far possible that internal examination might be prohibited altogether; but, though the danger of infection would thus be reduced to a minimum, faulty diagnosis would so frequently occur that the renewed introduction of internal examination would again take place as the lesser of two evils.

It is all the more fortunate, therefore, that a means exists of making the internal examinations which are necessary for a proper diagnosis without incurring danger of infection.

Different hands vary in the extent to which they can be disinfected, but many investigations have shown that no hand whatever can be completely freed from all organisms, whatever the labour and care taken.

In my opinion, therefore, reliance should not be placed on hand disinfection, and we should make it a routine practice to conduct all vaginal examinations with a gloved hand.

In making such examinations gloves are not liable to be torn and infected fingers exposed as they may be during operations, and the manufacture of very thin gloves does away with the objection against their use, that the sense of touch is diminished and the examination thereby made more difficult. It requires only very slight practice, and we have been fully convinced in the case of the numerous students in our hospital, as well as of those studying midwifery, that they can feel just as well as without gloves. The difficulty that midwives experience in putting on thin gloves I have endeavoured to eliminate by constructing a two-fingered examination glove, which protects the examining fingers in the ordinary way and the rest of the hand by a kind of mitten. With this simplification, which renders it perfectly easy to put on, the cost is also reduced and that further objection to their use done away with. These two-fingered "touching" gloves* are disinfected in the wholesale manufactory by streaming superheated steam and then packed in a triple cover, impervious to air and bacteria, in a way directed by me. I think that no valid objection can now be made to the universal use of this very important means of preventing the danger of internal examinations.

If we avoid too refined measures of disinfection on the one hand, and on the other deprive the necessary vaginal examinations of their danger, for which purpose the use of the touching glove is not only the simplest, but also the surest means, the result everywhere, in the statistics of all countries, must be to reduce the morbidity and mortality of puerperal fever to a minimum.

* To be had of Zieger and Wiegand, Rubber Manufacturers, Folkmarsdorf, Leipzig, Kirchstrasse.

REFERENCES.

[1] *Arch. f. Gyn.*, Bd. lxxix, p. 325. [2] *Zentralbl. f. Gyn.*, 1910, vol. v, p. 14.

DISCUSSION.

Dr. BALLANTYNE (Edinburgh) agreed with Professor Döderlein in practically all the remarks he had made, and followed, with one or two exceptions, the methods he had recommended. The difficulty which had always faced him was the almost absolute impossibility of applying *all* aseptic precautions in the serious cases of midwifery—for example, third stage haemorrhage, and the risk of leaving out essential protective details. So it often came about that obstetricians took the most complete precautions in the cases that needed them least, and ran the risk of omitting them in the cases in which they were most essential. He would counsel the young obstetrician not to feel that because there was no time to fulfil the whole aseptic technique he could omit all precautions. He regarded the use of rubber gloves to extract pieces of membrane as very difficult.

Dr. PURSLOW (Birmingham) had used gloves for operative midwifery, and found that there was no difficulty, except in cases of removal of adherent placenta. He believed British obstetricians were generally agreed that preliminary disinfection of the vagina in midwifery cases was unnecessary, but there was disagreement as to the advisability of preliminary cleansing of the vagina before performing Caesarean section. Personally, he did not consider it necessary, unless there had been preceding vaginal manipulation. The textbooks written for midwives did not draw sufficient attention to the extreme importance of keeping their hands, as far as possible, from contact with infectious matter in their daily work, and the midwives formed the idea that contact with all forms of organic and inorganic matter was equally dangerous.

Dr. HERMAN (London) quoted Dr. Kocher's operative surgery to the effect that the use of india-rubber gloves to the surgeon was to wear them when he was not operating. The expense was prohibitive to the poor midwife. The very best results had been attained without gloves.

Mr. S. G. KIRKBY-GOMES (Straits Settlements) said that in the East, where his own work lay, asepsis was even more difficult to obtain than in this country, and consequently gloves were largely used both in surgical and obstetric work. Much of his own success in both directions he could attribute to the use of gloves. He had not experienced the mechanical difficulty reported by those who spoke against the use of gloves in surgical practice, and firmly believed that with a little practice nobody else should either. Gloves, he thought, should be worn in all surgical procedure put into practice by surgeons, obstetricians, and gynaecologists alike.

Dr. FREDERICK EDGE (Wolverhampton) said that it was in the third stage that infection was most commonly incurred. This failure in the management of the after-labour stage was due to over-anxiety about shreds of placenta and membranes being left in the uterus, and consequent unnecessary interference with the uterine cavity. The greatest reluctance to interfere with the uterine cavity should be strongly inculcated by teachers. Meddlesome douching, and especially douching with inadequate cleansing of the vagina and vulva, should be specially forbidden. If the uterine cavity had to be entered, the procedure should be carried out as carefully as an abdominal section.

Dr. HASTINGS TWEEDY (Dublin) said that there was no operation in obstetrics and gynaecology that could not be performed with rubber gloves. The most adherent placenta could thus be removed, and to facilitate this, finger-tips could be cut away, and the bare tips painted with "new skin." If the glove were filled with methylated spirit, it slipped on to the hand with great ease. The spirit hardened the epithelium, and prevented the maceration of skin, therefore it was a mistake to suppose

that a glove must contain or did contain pools of septic fluid. Some complained that spirit blistered the skin, but the blistering was due to the incomplete removal of the antiseptic by spirit before the hand was placed in gloves.

Dr. Blair Bell (Liverpool) said he was sorry that a discussion on puerperal infection should resolve itself into a discussion of the advantages and supposed disadvantages of rubber gloves. The question had passed beyond a matter of judgement or opinion. The surgeon and obstetrician who did not wear them was exposing his patients to unjustifiable risks, and could not possess an aseptic conscience. If there were difficulty in removing an adherent placenta it could be overcome by putting a sterilized cotton glove over the rubber one. He called attention to the fact that surgeons were not nearly particular enough about the cleanliness of their hands in everyday life. No surgeon should do gardening or clean motor cars with uncovered hands. Further, he thought the habit of wearing leather gloves week by week an extremely dirty one—every one was more particular about his footwear. Personally he always wore white washable cotton gloves which he had had specially made. Such details of personal cleanliness should come natural (aseptic conscience) to the aseptic surgeon.

The President (Mrs. Scharlieb) said it was pleasant to learn that Professor Döderlein disapproved of the so-called preliminary disinfection of the birth passages, because in England it was believed that antiseptic douches or gauze scrubbing of the vagina were likely to do more harm than good. His suggestion of the limitation, but not the abandonment, of internal examinations, and the wearing by the obstetrician of india-rubber gloves, or, in the case of midwives, of a special form of protection for the half-hand, seemed to her wise. Several speakers had enlarged on the difficulty of removing scraps of placenta and fragments of membrane when wearing gloves—a trouble which others thought to remedy by wearing a sterilized cotton glove over the rubber. She did not think this difficulty was very great, especially if a roughened variety of glove were worn. It seemed advisable that when the whole hand had to be introduced into the uterus the glove should reach up to the elbow. Some speakers feared that the wearing of rubber gloves would tend to cause carelessness in the disinfection of the hands, but surely hands should be disinfected as carefully as if gloves were not going to be worn, and the gloves should be worn as if no attempt to cleanse the skin had been made. The suggestion of filling the gloves with methylated spirit to ensure their sterilization and to facilitate the introduction of the hands would certainly not suit all skins, and if the obstetrician got his hands sore or rough there was an end to the possibility of surgical cleanliness for some days to come. Probably glycerine that had been boiled was a safer lubricant.

ADDITIONAL CASES OF RUPTURE OF THE UTERUS.

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This short communication has special reference to three additional cases of rupture of the uterus upon which I have operated during the last two years. In the *Journal of Obstetrics and Gynaecology of the British Empire* in July, 1908, I recorded 14 cases. In that paper I referred in considerable detail to the symptomatology of the condition. On the present occasion, however, I shall only refer to the treatment.

Case I.

Mrs. F., aged 37 years, 4-para, was admitted to my ward in the Glasgow Maternity Hospital on March 31st, 1909. Her first three children were born without difficulty. She was seen by the house-surgeon twenty-four hours after labour had started. Forceps had been tried, but failed. On her arrival at the hospital she was in a collapsed state. The vagina was full of blood clot; the fetal limbs could be palpated easily on one side of the abdomen and the uterus on the other. When the abdomen was opened, it was found that the head was still in the uterus and the trunk free in the abdominal cavity. There was difficulty in getting the child's head through the rent in the uterus. Complete hysterectomy was performed, and the left parametrium drained per vaginam by means of gauze and tube drain. The patient made an excellent recovery. The child weighed 12½ lb.

Case II.

Mrs. S., 7-para, admitted on October 10th, 1909, in a very collapsed state. The child's body was born, but the head remained above the pelvic brim. Several futile attempts at delivery were made. When sent into hospital, it was recognized that the head was hydrocephalic. The head was therefore perforated and easily extracted. The child had spina bifida. After the birth of the child the hand was passed into the uterus to remove the placenta, and a tear was discovered, extending up the whole of the left side. The abdomen was opened, and an incomplete tear discovered; the peritoneum was intact. The abdomen was closed and the tear packed with gauze per vaginam. The patient died in five hours.

Case III.

Mrs. R., aged 38 years, multipara, was admitted into hospital in a collapsed condition. The child had been in the transverse position, with arm prolapsed and shoulder impacted. Internal version attempted before admission. On examination a large mass was found on the left side of the abdomen, and in front and to the right of it another less movable one could be palpated. Per vaginam, the vagina was full of blood, and no fetal parts could be felt. Abdomen opened. A large complete tear, involving cervix and lower uterine segment, and extending to the insertion of tube, was discovered. Hysterectomy; left parametrium drained. The patient made an excellent recovery.

Remarks.

It is a matter of great satisfaction that the results obtained from operative treatment have greatly improved in recent years. Since my paper dealing with fourteen cases which I had treated appeared, the most valuable contribution in English on the subject is Eden's paper read before the Royal Society of Medicine on May 13th of last year. Eden strongly supported operative treatment, and in the three cases he detailed was able to record two recoveries. There is little doubt that in the slighter degrees of incomplete rupture plugging still gives the best results, but in the severe cases of complete rupture abdominal section is pretty generally admitted to be better. Some really very brilliant results have been obtained in recent years by such treatment, as will be seen by consulting the table compiled by Petrén of Lund.[1]

The majority of cases of rupture of the uterus operated upon by abdominal section, which die, die of sepsis, not of shock. Taking my own cases, I have performed hysterectomy 11 times with 5 recoveries—that is to say, a mortality of 55 per cent. Now of the 6 fatal cases, 4 died of sepsis and 2 from shock.

Putting aside the cases of slight incomplete rupture, let us consider only the graver forms of rupture and the most satisfactory methods of dealing with them. Since I commenced to perform abdominal section for such cases my results have steadily improved. The first 6 I tried by supravaginal amputation, and with that treatment I had 4 deaths—a mortality of 66 per cent. In these 6 cases I was struck by the fact that 3 of them died, not of shock but of sepsis. I therefore determined to try and improve my operative treatment so as to lessen this. Thinking the matter over, it appeared to me unsound to leave behind the cervix, the part of the uterus that in many cases was probably chiefly infected by the operative procedures and examinations that had taken place prior to the rupture. I therefore determined to remove the cervix. In this way I treated 3 cases, but still had 2 deaths—exactly the same percentage as with supravaginal amputation. I therefore thought I should go a step further, and, in addition to removing the cervix, that I would drain the lacerated broad ligament and completely cover the lower part of the pelvis with peritoneum. My last 2 cases I have treated in this way, and both of them have recovered. The drain used has been a thick rubber tube brought out through the vagina and loosely surrounded with gauze. I am in the habit of leaving this drain in for three or four days. As far as my experience goes, this is the treatment I have found most satisfactory.

Reference.
[1] *Monats. f. Geb. u. Gyn.*, Bd. 29, Heft 3, 1909.

AIR EMBOLISM DURING LABOUR.

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Air embolism probably occurs more frequently during labour than is commonly supposed. Very little attention is paid to it in most of the ordinary textbooks, and the

practitioner is thus led to look upon it as so very rare that the possibility of its occurrence escapes his memory, and he fails to recognize a case when he meets with it. While the accident is admittedly rare, the prevention of it is of so much importance, and the treatment of it requires to be so definite and prompt and decided, that the subject is worth more attention than it generally gets.

Symptoms.

The symptoms are usually described as being similar to those of pulmonary embolism from clot. No doubt this is the case where the condition is of an extremely grave character and proves suddenly fatal. Here recognition of the cause of the trouble is not of so much importance, because the end comes so quickly that nothing can be done to save the patient. Cases which are of the milder type and have a chance of recovery under treatment show symptoms which are by no means like those of pulmonary embolism. In fact, the difference is quite remarkable. Instead of a sudden and severe attack of spasmodic inspiratory efforts, with perhaps convulsions and sudden death, as occurs in the case of the worst forms of both air and blood embolism, there are a series of recurrent attacks of dyspnoea. The recurrent character of the spasms, and the fact that intervals of comparative well-being are for a time to be observed, serve to differentiate the air embolism from the blood embolism.

In the case of the ordinary pulmonary embolism the clot gets driven into that part of the pulmonary circulation in which it becomes wedged, and remains there, producing infarction and giving rise to a train of symptoms which retain their urgent character for a considerable time and gradually subside. The worst of the attack is at the onset if the patient has any chance of recovery, and symptoms slowly become less pronounced with lapse of time. On the other hand, while the onset of an attack of air embolism resembles that of blood embolism, its future progress is quite different in cases that recover. The spasmodic inspiratory efforts and the convulsions, if such be present, pretty rapidly pass off. The patient seems but little the worse. In half an hour or so the attack is repeated. Again it is over in a few minutes. Again the patient seems comparatively well for a time. Soon another seizure follows, and the whole thing is gone over again; but after each attack the patient gets more exhausted. The cyanosis does not completely pass off in the intervals. The pulse becomes gradually more frequent and more feeble. The heart becomes more and more dilated. Ultimately death takes place if the condition has not been relieved. This tendency for acute symptoms to comparatively rapidly pass off, and to recur again and again, is what one would naturally expect in the case of air.

The blood clot forms a permanent block in that part of the circulation into which it has been driven, while the air has a tendency to get broken up or to escape through the lung tissues. The air lock has thus a natural tendency to disappear somewhat rapidly. In that respect we might look upon it as not so serious as blood clot. On the other hand, air embolism is prone to recur again and again, and ultimately wear out the patient, unless it be recognized and energetically treated. This tendency to recur, which is so characteristic of air embolism, is, as we shall see later, due to the recurrent contractions of the uterus. These elementary points have been so much dwelt on because they bear both upon preventive and remedial treatment. In the case of pulmonary embolism from blood clot but little effective treatment is possible. The patient must fight it out for herself to a large extent. In the case of air embolism treatment is all important, because without active interference by the medical attendant death will in many cases follow in spite of the best efforts of Nature, and will indeed be hastened by those efforts.

Cause.

The causes assigned as being concerned in the production of air embolism during labour are numerous, but practically the only important cause is the delivery of the patient under anaesthesia in the left semiprone position and failure to turn her over on her back as soon as the child's head is born. Retention of the placenta within the uterus is a necessary factor in causation. When a patient is delivered in the left semiprone position under anaesthesia expulsion of the child is immediately followed by an inrush of air into the vaginal and uterine cavities. This is brought about partly by gravitation of the pelvic organs, as a result of the semiprone position, and partly by the relaxation of the uterine walls. So long as the placenta is not detached, the entrance of air will not produce embolism. When, however, the placenta has become detached and has got driven down into the cervix, the danger to the patient begins as soon as any air gets above the placenta. The uterine cavity then contains a certain amount of air. The placenta in the cervical canal acts as an effectual cork, holding the air imprisoned within the uterine cavity. In these circumstances a uterine contraction is bound to compress the air. The air will escape by the path of least resistance. This path is most likely to be through the venous sinuses in the placental site, and thence along the veins to the right side of the heart, and so to the pulmonary artery. The embolism will thus show itself during a uterine contraction or during an attempt to express the placenta. In a non-fatal attack the symptoms will soon pass off, the air getting broken up or absorbed in the lung. As soon, however, as another uterine contraction comes on or another attempt at expression of the placenta is made, the symptoms reappear with all their original force. These phenomena will be repeated until all the air in the uterine cavity is disposed of in the circulation, or until the placenta is removed, or until the patient has succumbed to numerous recurring paroxysms. Attention to these elements in causation will easily convince the obstetrician of the importance of preventive treatment and of the necessity for a prompt and definite line of action when the symptoms have appeared.

Prophylaxis.

Preventive treatment consists in completing the delivery of the child's body with the patient on her back, and in performing all obstetrical operations with the patient on her back. The possibility of air embolism occurring is practically banished by adhesion to these simple rules. The possibility of air embolism following delivery in the left-side position is a very strong argument against the posture which is usually adopted by English-women during delivery.

Treatment.

When air embolism has taken place the placenta should immediately be removed by hand. Attempts at expression are not only useless but positively dangerous. They are very likely to force more air into the venous sinuses, and, as the influence they exert on the placenta must be through the volume of compressed air, they can have but little effect in dislodging the placenta. The air in the uterus is more likely to enter the venous sinuses than to transmit the force to the placenta. When the placenta has been removed by hand, the uterus should be washed out with a large flushing loop curette and plenty of sterile saline solution.

When a number of spasmodic attacks have occurred ancillary measures may be of service in enabling the patient to recover from her state of exhaustion. Saline infusion, the hypodermic injection of strychnine, the use of oxygen, and the administration of digitalis have an important place in this connexion. It must, however, be clearly recognized that removal of the placenta by the hand introduced into the uterus takes precedence of all other methods of treatment, and is the first thing to be done.

After-effects.

When the immediate anxieties have been overcome a long course of after-treatment will be required. Above all things, rest in the recumbent position for five or six weeks should be insisted on. The dilated heart requires time to recover from the strain. Anaemia is mostly a pronounced feature and will require attention.

After-effects do not seem to be noticeable even in severe cases. In course of time the patient regains her usual health and goes successfully through her future pregnancies. The acute dilatation of the heart subsides. Any murmurs noticeable during convalescence disappear. While it is unlikely that the organism can go through such a terrible ordeal as air embolism without some permanent damage, nevertheless no appreciable effect is left behind.

The following two cases fairly illustrate the accident of air embolism occurring during labour. One was more

severe than the other, and nearly proved fatal. The other was moderate in severity. Both recovered under treatment.

CASE I.—Severe. The patient was a strong, well-made, healthy woman. She had had one child, delivery being normal and unaided. Her second confinement was also normal and unaided throughout the first and second stages. She lay on a narrow bed with a wire mattress, which was much depressed in the centre. Consequently when she lay on her left side the semi-prone position was considerably exaggerated. She had a little chloroform during the later stages of the descent of the head, the anaesthesia being fairly deep during the expulsion of the head. She was not turned upon her back until after the child was completely born. Within a few minutes of the birth of the child she had completely regained consciousness and looked and felt perfectly well. With the onset of a uterine contraction she exclaimed that she was dying, made several intense spasmodic inspiratory efforts, became cyanosed, and had a general convulsive seizure ending in opisthotonos of the most pronounced description. The pulse became frequent, feeble, and irregularly intermittent. The spasm almost immediately passed off, and in a few minutes she felt well and looked none the worse. Attempts to express the placenta were made about twenty minutes later, and were followed by a uterine contraction which brought on the same series of events as before. Several uterine contractions followed at intervals of fifteen to thirty minutes, and each of them formed the starting-point of a new spasmodic and convulsive seizure. The cyanosis soon became permanent, though deepening during the spasm. The pulse was in every respect worse. The dilatation of the heart became very noticeable. Vomiting occurred at intervals and gave slight temporary relief. The patient appeared to be at the point of death. At this stage the placenta was removed by hand and the uterus washed out with saline solution. A vein in the arm was opened and a pint of saline run in. Strychnine was given hypodermically and small quantities of hot milk and a little whisky administered. After removal of the placenta no further spasmodic attacks came on. The colour and general condition slowly and gradually improved. Vomiting took place several times during the first three hours after removal of the placenta. The temperature never rose a point, but the pulse remained at 100 for about two weeks. At the end of four weeks the patient was apparently quite well though still anaemic. Six weeks after the confinement she was able to go about. Since then she has had a normal labour, and is now in good health and apparently not in any way permanently affected by the experience through which she passed.

CASE II.—Moderate in severity. The patient had always been a delicate girl, subject to anaemia and easily run down. Labour proceeded normally until the head began to press upon the perineum. As progress ceased and she became exhausted, chloroform was given and delivery accomplished by forceps, the perineum being somewhat torn. She lay upon an ordinary double bed with wire mattress. The mattress sagged slightly in the centre, but not to an unusual degree. Delivery of the child took place in the left-side position, the hips being supported by the firm edge of the bed, while the knees and abdomen sank into the hollow of the wire mattress. After the child was born the first uterine contraction was followed by a pronounced spasm, marked evidence of air hunger, and frequency and irregularity of the pulse. These symptoms soon passed off, but the patient complained of severe pain in the region of the left shoulder-blade. Examination of this area by auscultation and percussion revealed nothing. Attempts to express the placenta were followed by uterine contractions and a return of the spasmodic symptoms. Vomiting took place several times. Dilatation of the heart became marked. Ultimately the placenta was removed by hand, and the uterus washed out with normal saline solution. No spasmodic attacks occurred after removal of the placenta. Gradual and slow improvement went on, and apparently complete recovery ensued. This case differed from the preceding chiefly in the absence of cyanosis and severe convulsions. In neither case was there albuminuria.

INTRAPERITONEAL HAEMORRHAGE IN CASES OF UTERINE MYOMATA.

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[ABSTRACT.]

APART from operation, the escape of blood from the vessels of a uterine fibromyoma into the peritoneal cavity is so rare an event that textbooks and monographs are silent concerning it. Few cases are recorded in the literature, and some of these only in the briefest manner. Yet it has long been known that haemorrhage can take place directly from veins and sinuses in the tumour itself, probably the earliest instance being that reported in 1866 by Matthews Duncan, in which death followed bleeding per vaginam from a large vessel in a submucous fibroid. It is also known that a cystic fibroid can bleed into its own cavities, especially after torsion of the pedicle of a pediculated fibroid. Specimens of subserous fibroids have been exhibited—notably by Treub and H. R. Spencer—which show varices on their peritoneal surfaces. Rupture of such varices, or laceration occurring in a tumour the seat of degenerative changes, may lead to serious intraperitoneal haemorrhage.

Such an accident occurred to a patient under the writer's care in the Hospital for Women.

She was a single woman, aged 31, who had always enjoyed good health, and in whom the existence of a fibroid had never been suspected. Early in the present year she began to suffer from "bilious attacks," and then on March 23rd, after severe exertion, acute abdominal pain with vomiting came on. After resting for a day the attack passed off, but ten days later a second followed the act of sitting up in bed to consult a watch. Some hours later the patient's medical attendant saw her and sent her into hospital. When admitted the temperature was normal and the pulse-rate 76, and there was no sign of internal haemorrhage. The abdomen was tumid and tender, and a hard tumour projected upwards out of the pelvis. The provisional diagnosis was an ovarian tumour—probably a dermoid cyst—the pedicle of which had undergone torsion.

Laparotomy revealed a fibroid which sprang from the posterior wall of the uterus. About 6 or 7 oz. of dark red fluid blood were free in the peritoneal cavity. A small projection was felt on the left side of the tumour, but no particular attention was paid to it. Myomectomy was performed and the uterus was reconstructed. So far the most careful exploration had failed to disclose the source of the bleeding, and now a fresh and minute search was no more successful. The uterine appendages were normal, and there was no recent ruptured follicle on either ovary. The abdomen was closed and the patient sent back to bed. It was only now, whilst handling the tumour, that the origin of the bleeding became clear. Mention has been made of a small projection on the otherwise smooth rounded surface of the mass; from this blood was noticed to be oozing. Closer investigation showed that the projection was situated immediately over a large superficial vein that coursed visibly beneath the peritoneum. Gentle pressure on the vein was followed by further oozing of blood from the projection, and on the summit of the latter was visible a small rent with thinned and ragged edges. The projection was therefore a varicosity of the superficial vein; the wall of the varix had yielded under the increase of blood pressure caused by severe exertion, and intraperitoneal bleeding had occurred, fortunately for the patient, only in limited amount. After the specimen had been hardened the continuity between vein and varix was further established by the passage of a probe. The patient recovered and returned home on the ninth day.

Rokitansky (1861) was probably the first to mention this condition, but he merely stated that "the tearing of the superficial vein of a fibroid tumour, with bleeding into the peritoneal cavity, has been observed." Five cases have been recorded in which the diagnosis was not verified either by operation or autopsy. Albert Martin, Amann, and Albrecht have each mentioned instances in the briefest manner, those of the latter two being rapidly fatal. With similar brevity, cases have been mentioned by H. R. Spencer, A. H. N. Lewers, Mrs. Scharlieb, Döderlein, Pean, and Gusserow and Zweifel.

Jaschke recently recorded an instance of bleeding from a varicose vein in a woman of 43. This is the only known instance in which a diagnosis of intraperitoneal bleeding from a uterine fibroid has been made prior to operation. An immediate and rapid hysterectomy failed to save the patient.

The existence of a varix is not essential for the occurrence of bleeding, for in three fully reported cases the rent has been found in the wall of an apparently normal vein (Stein's and Tedenat's two cases). In two cases (A. H. N. Lewers and R. M. Littler) there have occurred in myomata superficial lacerations involving blood vessels. In Lewers's case the presence of fresh blood in the peritoneum and the existence of the laceration in the myoma were totally unexpected. In Littler's, the injury was caused by a fall, which was followed by an acute peritoneal crisis.

A very rare cause of bleeding must be ulceration of the surface of the myoma as the result of pressure on the tumour, as in the case reported by W. Bruce Clarke. The tumour was wedged against the sacral promontory, and the area pressed upon by the promontory became ulcerated. When the abdomen was opened an artery and vein were bleeding at the site of ulceration. Finally, mention may be made of rupture of a superficial sinus as the result of increase of local blood pressure consequent on torsion of the pedicle of the fibroid, as in the case recently recorded by Steinbüchel.

Diagnosis.

In several cases operation has been undertaken for myomata only, the occurrence of intraperitoneal bleeding

being unsuspected. In other cases the complication has been mistaken for dermoid cyst with torsion of the pedicle; for perforated gastric or duodenal ulcer; for tubal gestation. In two cases the patients suffered from inexplicable and advancing anaemia that could not be explained by external losses of blood or by general disease. In both of these operation showed the cause to be oozing of blood from superficial veins of myomata. When the losses of blood have been in greater amounts, the patients have a history of repeated peritoneal crises, as in Pean's case. The diagnosis depends on the existence of a myoma, *plus* the symptoms and signs of internal haemorrhage. In some cases it may be possible to recognize the presence of free fluid in the peritoneum, but this implies great shedding of blood. In every case the possibility of the occurrence of ectopic gestation in a patient suffering from a myoma requires consideration. Cases may be divided, roughly, into two classes:

1. The fulminating, in which the escape of a large amount of blood into the peritoneal cavity is followed by an acute peritoneal crisis and collapse.

2. The subacute, or even chronic, in which the oozing of small amounts of blood, or of the escape of very moderate quantities, are evidenced either by inexplicable anaemia, or by mild signs of peritoneal disturbance of short duration.

Mortality.

In 16 cases the termination of the case has been stated after operation or autopsy. Eight of these are briefly, 8 fully, recorded. Of the former 3 died, of the latter 2. Five deaths out of 16 patients yields a mortality of 31.25 per cent., a rate that indicates how serious the condition is.

It is scarcely necessary to add that in every case immediate laparotomy is indicated.

LOCALIZED NECROSIS OF AN ADENOMYOMATOUS RIGHT TUBE, WITH A LEFT TUBAL MOLE AND FIBROID UTERUS.

By FRANCES IVENS, M.S.Lond.,
Liverpool.

THE patient in the following case was a woman aged 31. She had been married eighteen months, her periods before marriage being regular, not profuse, and lasting two to three days. She had married in November, 1908, and menstruation continued to be regular until February, 1909, and then ceased for three months.

First Illness.—In May, 1909, flooding with the passage of clots occurred, but stopped with rest in bed. As the abdomen continued to increase in size and a further period of amenorrhoea ensued until the following November, the patient imagined she was still pregnant, and having felt what she thought were fetal movements, expected her confinement in November. Instead, however, a dark red discharge began lasted for three weeks, and was followed by a further period of amenorrhoea. On March 1st, 1910, haemorrhage with the passage of clots again occurred. Between this date and the preceding November the patient thought her abdomen had decreased in size.

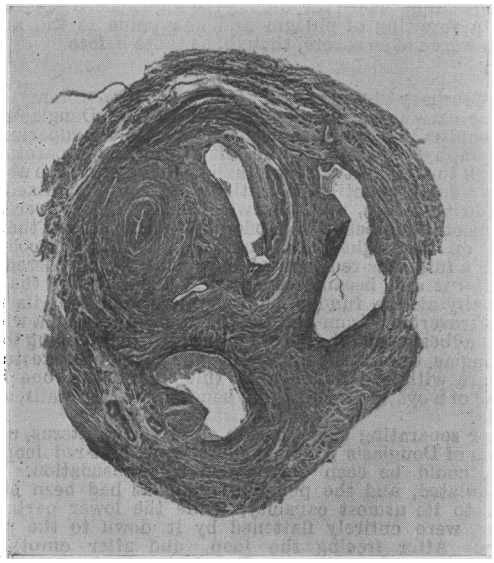

Fig. 1.—Section taken through the adenomyomatous nodules showing the normal lumen pushed to one side by a mass of unstriated muscle fibres in which gland spaces are scattered irregularly.

Admission to Hospital.—As the haemorrhage and passage of clots continued, the patient, after a fortnight, was sent to the Stanley Hospital by Dr. Huey, of Millom, Cumberland, being admitted on March 14th, 1910.

State on Examination.—She was a short, stout woman of rather unhealthy appearance and sallow complexion. There was an extremely thick layer of abdominal fat, and a satisfactory examination was impossible. Bimanually a tender, ill-defined swelling could be felt in the left iliac region, and the uterus was enlarged and rather hard. There was a copious dark red discharge.

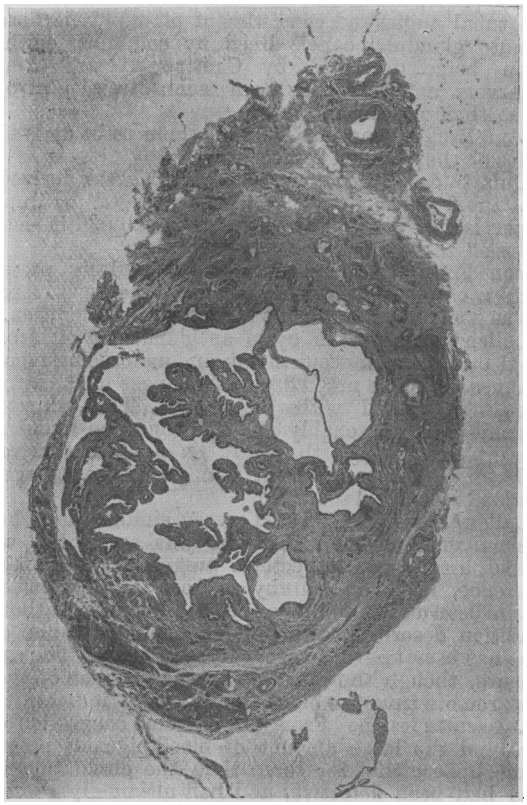

Fig. 2.—Localized necrosis in adenomyomatous tube. The section is taken through the edge of the perforation, and shows thinning of the mucous membrane at one point.

Operation.—A diagnosis of ectopic gestation having been made, on March 17th I opened the abdomen by a median subumbilical incision. The stomach was very much dilated, being dragged down by an omental adhesion which was firmly fixed to the left tube. After separating this, the tube was seen to be closed and distended, dark red in colour, and firmly adherent to a cystic left ovary. Both were removed. There was no haematocele, though it was evident that the omentum had adhered to a rupture about the middle of the tube. The uterus was rather large and thick, and a small subperitoneal fibroid was enucleated from the fundus. On drawing up the right tube a small circular perforation was seen in its middle third, opposite to the attachment of the mesosalpinx. The edges of the opening were greyish brown in colour. The tube itself was thickened and not dilated, though the fimbriated end was closed and rather bulbous-looking. At the uterine end of the tube, close to the cornu, was a nodular thickening whitish in colour, and not exceeding ½ in. in diameter. The tube was removed, but as the ovary looked healthy it was left *in situ*.

Parts Removed.—The left tube measures 5 in. along its convexity, and the lumen contains firm dark clot in which are a few degenerate villi. The right tube measures 2½ in. A section through the nodule shows a normal lumen, pushed rather to one side by a mass of unstriated muscle fibres in which are scattered, irregularly, gland spaces varying in size (Fig. 1). These are lined by a single layer of low columnar epithelium, which in some lies directly on the muscle, while in others a densely infiltrated stroma resembling that of the uterine mucosa intervenes. In the gland spaces are red blood corpuscles, desquamated epithelium, and leucocytes laden with blood pigment. A section taken through the edge of the perforation shows thinning of the mucous membrane at one point, with intensely congested vessels, and cloudy swelling of the epithelium which has partly disappeared (Fig. 2). Masses of blood pigment are scattered through all the layers of the tube wall. The lumen shows no sign of previous dilatation. Separated from the tube lumen by a thin layer are several gland spaces containing desquamated epithelium, blood, and coagulated serum. They are not surrounded by muscular tissue.

Progress.—The patient's convalescence was uninterrupted, but on returning home further uterine haemorrhage occurred, and she was readmitted for hysterectomy.

Readmission.—On June 2nd I removed the uterus by a supravaginal operation. It was interesting to note that the stomach had regained its normal size. The uterus was thickened generally, and especially the posterior wall, but no encapsuled tumours could be seen. The mucous membrane was thicker than usual, but no glands were seen in the deeper muscular layers. The patient made a good recovery.

REMARKS.

The presence of small tumours in the tube situated near the tubal angle, and consisting of plain muscle fibres and minute glandular canals lined by columnar epithelium, have been described by Continental and American observers, and there has been much diversity of opinion as to their nature and origin.

Von Recklinghausen[1] believed them to be derived from relics of the Wolffian bodies.

Chiari[2] described them as isolated cysts derived from the tubal mucous membrane surrounded by muscular hyperplasia, under the title of "salpingitis isthmica nodosa."

Von Franqué[3] found chronic tuberculosis present in such a case, and on this account regards tuberculosis not as a complication but as a cause of the condition.

Cullen[4] regards the cysts as glands cut off from the tubal or uterine mucous membrane undergoing menstrual changes. He has noted decidual changes in the stroma in a case associated with pregnancy. This view would account most reasonably for the condition found in the present case, where there are no signs of tuberculosis, and where the contents of the gland spaces resemble retained menstrual secretion.

Cullen[5] has also described a very similar case where an adenomyoma of the right tube was associated with a fibroid uterus and left ectopic pregnancy. He makes no reference, however, to any necrosis in that case, and I have been unable to find anywhere a similar pathological condition described, although complete gangrene of the tube has been known to occur from torsion.[6] The cause is obscure, though the situation in that portion of the tube most remote from the blood supply would indicate that it is a vascular lesion. From the extreme congestion of the tube and the large amount of blood pigment present, it would appear that for some time the circulation in the tube had been defective, and had ultimately resulted in complete stasis and necrosis.

As far as could be ascertained, no infection of any kind was present. No organisms could be demonstrated in the tissues, no adhesions were present, and microscopically there is complete absence of any round-celled infiltration indicating an inflammatory origin. The fibroid uterus and ectopic pregnancy of the left side may have been contributory factors in increasing the congestion.

In analysing the history it is somewhat difficult to explain the prolonged periods of amenorrhoea alternating with profuse haemorrhages, but it is conceivable that two early miscarriages followed by a tubal pregnancy occurred, the final haemorrhage taking place coincidentally with an intratubal abortion.

REFERENCES.

[1] V. Recklinghausen, *Die Adenomyome Cystadenoma den Uterus und Tubenwandung*, 1896. [2] Chiari, Zur pathologische Anatomie des Eileiterkatarrh, *Prager Zeitsch. f. Heilkunde*, vol. viii. [3] Von Franqué, Salpingitis nodosa isthmica und Adenomyoma Tubae, *Zeits. f. Gebürts. und Gyn.*, Band xlii, S. 41. [4] Cullen, *Adenomyoma of the Uterus*, Philadelphia, 1908, p. 246. [5] Cullen, ibid. [6] Sänger, Ueber hämorrhagische Tubennekrose, *Zentralbl. f. Gyn.*, 1893, p. 727.

CASES OF INTESTINAL OBSTRUCTION DUE TO THE UTERUS.

By Professor SIGISMUND GOTTSCHALK,
Berlin.

I.—OBSTRUCTION DUE TO A FIXED RETROVERTED NON-PREGNANT UTERUS TREATED BY LAPAROTOMY.

THE patient in the following case was a woman, aged 31, who had been six years married, but had no children. She had had one miscarriage five years previouly, and had undergone a laparotomy in Königsberg four years ago for a right ovarian cyst, the size of an orange; there had also been hydrosalpinx of the right tube, which was treated by salpingostomy. The position of the uterus was then quite normal, and she made a good recovery.

Since the operation, patient sometimes complained of pains in the lower abdomen, constipation alternating with diarrhoea; menstruation was irregular and painful, from eight to ten days' duration, great loss of blood, sometimes accompanied by headache.

On February 18th, 1910, sudden intensification of pain in the lower abdomen, passing soon, but a tiredness remained until February 20th, 10.30 p.m. At this hour there suddenly appeared very violent pains in the abdomen, which became weaker at times, but which returned with increased severity and lasting all night. At first the lower abdomen alone seemed affected, but gradually the pains mounted higher, and finally there was a very strong cardialgia. Soon a slight diarrhoea appeared, after which the pains increased almost to causing unconsciousness. Two hours later a strong nausea appeared, followed by violent vomiting. I heard a few days later that the vomited matter had a decided excremental odour. At 1.30 a.m., on account of the grave condition of the patient, a physician was called, who prescribed morphine cones, which, however, did not bring the least relief. On the following morning, 8.30, Monday, February 21st, I was called to the bedside.

State on Examination.

I found patient collapsed, with thin, frequent pulse, alarmed expression, and writhing in spasmodic pains of the abdomen, reaching as far as the stomach, while backache was present as a reflex pain. The lower abdomen on the right only showed slight swelling, but was very sensitive to the touch; increased peristalsis was indistinct. I found on examination the uterus retroverted and behind; within the peritoneal fold (Douglas) I found a diffuse resistance, in which liquid and solid parts could be felt, similar to a fresh haemorrhage.

Although one could be inclined to think of an internal haemorrhage on account of this result of my examination and the collapsed state, the livid colour of the lips were against such a diagnosis, besides the fact that the menstruation had appeared in time.

On account of the grave condition of the patient I had her removed in an ambulance to my private clinic, where I examined her again immediately.

Diagnosis.

The result was that I did not think an internal haemorrhage on account of extrauterine pregnancy had taken place, for the mass in Douglas's pouch could not be felt any more now, although at the previous examination it had felt to the touch like a fresh haemorrhage. Instead of that I now felt within the peritoneal pocket, behind the retroverse fixated uterus, an elastic excrescence, shaped like a sausage, extremely sensitive to touch. After I had given 1 grain of opium per rectum, in order to overcome the pains, I tried carefully to accomplish a reposition of the strangulated vermiform growth. By manipulating it, it gradually became less distinct.

The contents of the bowel could be pushed gradually to the right, upwards. Two high camomile enemas then induced stool. After that appeared a notable improvement; in the evening the pulse went down from 120 to 96, became stronger, and the abdominal pains decreased.

In spite of the fact that I now had no doubt that there had been a strangulated bowel within Douglas's pouch, one could think, after the favourable turn had set in, that the strangulation had been overcome now by my attempts of reposition. At any rate, I now could conscientiously attempt to wait until the next morning. However, during the night there appeared once more a vomiting of phlegm and new pains in the abdomen, which were not so severe, though, as those before.

Operation.

On February 22nd, at 8 a.m., I performed laparotomy, diagnosing the case as a strangulated bowel within Douglas's pouch. The longitudinal cut from the umbilicus to the superior part of the symphysis passed to the left of the old laparotomy scar through the left rectus abdominalis. In opening the abdominal cavity a small quantity of ascitic liquid ran out, mixed with a few fibrine flakes, but otherwise clear. There were no adhesions of intestines to the abdominal wall. Above the inferior pelvis on the right there appeared, within a bloody-brown liquid, a full, blue-red loop of the ileum, which passed behind the uterus and became invisible. The body of the uterus, especially at the fundus, showed bluish-red, too; its position was retroverted; from the fundus there appeared a wide peritoneal adhesive membrane of 1 cm. width, overlapping the pouch of Douglas, toward the upper anterior part of the rectum. On pressure with the finger upon this membrane, one had the feeling of a cystic growth lying beneath, of thin walls, and very elastic.

After separating the adhesion close to the uterus, upon the bottom of Douglas's pouch, a short, dark, blue-red loop of the ileum could be seen surrounded by transudation. It was strangulated, and the pouch of Douglas had been stretched by it to its utmost capacity, while the lower parts of the uterus were entirely flattened by it down to the posterior cavity. After freeing the loop, and after emptying the transudation, the widened Douglas's pouch resembled the interior of the bladder. At its left half, between the posterior wall of the uterus and rectum, it was surmounted by numerous low-embedded strong peritoneal adhesions, which mostly could be separated with the scissors, *while on the right, where there did not exist any adnexa, there were no adhesions.* On account of the superior peritoneal bridge of adhesions, there existed at this spot a hole, through which the

loop of the ileum had passed. The left Fallopian tube and the left ovary, which was partly degenerated by cystic tissue, were so closely grown together with the sigmoid flexure that it was hardly possible to separate them carefully. Considering that there existed a possibility that *Bacillus coli* already had entered the abdominal cavity from the strangulated bowel, it was resolved not to separate the left adnexa.

In order to prevent a repetition of strangulation, the mobilized body of the uterus was fixed to the abdominal wall, near the inferior angle of the wound. At this spot the peritoneal wound margins were fixed with one catgut suture symperitoneally with the middle of the anterior uterine wall, so that no space was left between the anterior corpus wall and abdominal wall. The old laparotomy scar was excised and a compression bandage applied.

After-Progress.

Temperature, being 37.6° before the operation, mounted up to 37.9° on the evening of the same day; on the next day it reached 39.5°, while the pulse went to 140 from 98. The pulse was thin; there were decided symptoms of cardial weakness. This danger was happily overcome by an infusion of 600 c.cm. of 0.9 per cent. NaCl solution into the left infraclavicular region, and two injections of caffein, natr. benzoic, furthermore 8 gutt. digalen. On the next day, February 24th, the temperature went down to 38.1°, the pulse to 120; after another twenty-four hours they had decreased to 37.8° and 100, and from that time on became normal again. The stimulation of intestinal activity made little difficulty; the day after the operation wind was expelled after an enema of camomile.

Result.

The abdominal wound healed by first intention, and on March 19th the patient was discharged as cured in a good state of health. On the fourth day after the operation the menstruation set in regularly.

REMARKS.

Four years ago the patient had undergone a laparotomy operation for right adnexal tumour, probably gonorrhoeal. The closed left Fallopian tube had been opened by salpingostomy in order to create a possibility for conception. The consequence was that on the left side numerous peritoneal adhesions formed, and the newly-formed ostium of the tube was again closed definitively through adhesion with the adjoined sigmoid flexure. The strong left peritoneal adhesions, easily inflamed, allow the deduction that from the artificial ostium infectious micro-organisms emigrated into the pelvic cavity in a secondary manner; the extended formation of adhesions had nothing to do directly with the laparotomy, otherwise, no doubt, they would have established themselves upon the right side, where the diseased adnexa were removed, and in the neighbourhood of the laparotomy wound. There was not the least sign of any adhesive formations. There remained a void space, because on the right side no adhesions formed; through this hole the loop of the ileum slipped into Douglas's pouch, which was surmounted by adhesions and by the retroverted and fixated uterus. The retroversion was formed in a secondary manner in connexion with the operation, no doubt on account of the pelvic peritonitis of the left side, for I heard from the surgeon who had performed the first laparotomy that the uterus had been found in a normal position.

The case is of importance, not alone through the strangulation of the ileum by the retroverted uterus, but because it shows that salpingostomy is not without danger under certain circumstances—namely, that inflammatory micro-organisms may penetrate through the new ostium into the pelvic-abdominal cavity. The case, therefore, serves as a warning example to be very careful with the indication for salpingostomy. It also shows how difficult it is to predict in advance whether an artificial tube ostium will remain open.

In regard to differential diagnosis, it should be remarked, furthermore, that the faeces within the strangulated loop, in the middle of the transudation surrounding the bowel, at first felt to the touch like a fresh haemorrhage.

Of great interest also is the marked improvement of the general condition induced by me through manipulation, attempting a reposition, followed by a high enema. Very likely this improvement may be explained in the following manner: During the manual attempts to obtain a reposition, the pent-up contents of the bowel were moved beyond the impediment, inducing a diminution of the abdominal pains and an improvement of the pulse by a movement of the bowels, which could be induced by those high enemas. The impediment, namely, the strangulation, however, remained. The patient was lucky in so far that the conspicuous improvement did not induce me to forego a laparotomy.

In this respect the case shows many interesting features, too, for it proves that a passing, pronounced, objective and subjective improvement by no means needs be synonymous with the overcoming of the trouble, in this instance the impediment.

II.—Obstruction due to a Retroverted Puerperal Uterus: Simulation of Peritonitis: Treatment by the Knee Elbow Posture.

The patient in this case was a secundipara, aged 33, with medium flattened pelvis. Her first labour, three years previously, was normal. During the last months of her second pregnancy she had much discomfort, and during the last weeks profuse suppurating discharge. Labour occurred spontaneously on March 29th, 1910, and after rupture of the membranes the midwife made a manual examination by means of a sterilized rubber glove.

On March 30th—second day of childbed—pronounced abdominal and dorsal pains, coming in jerks; the midwife diagnosed them as after-pains. The nurse had to force the patient to take food. In the afternoon there was vomiting of compact matter and singultus; jerking pains in abdomen and back, which increased in intensity; lochia stopped. Rectal enema of camomile without success. No faeces or flatus.

March 31st—third day of childbed. The general aspect became worse; vomiting and singultus; pain in the abdomen, still jerky, became so much more severe that injection of morphine did not bring relief, in spite of the fact that temperature did not increase; pulse 84. At mid-day I visited the patient for the first time in consultation with the family physician.

State on Examination.

A small, graceful woman, with a frightened expression; at intervals she writhed in severe pain in the abdomen and back. Lower abdomen strongly inflated as far as the umbilicus, and sensitive to pressure in its lower lateral parts. The corpus uteri cannot be palpated from the outside, and lies retroverted, surmounted by strong inflated loops of bowel. Temperature normal, pulse about 90; tongue is tolerably dry. Diagnosis arrived at, namely, commencing puerperal peritonitis; the relatively good pulse, however, appeared favourably but conspicuous. In puerperal peritonitis it is not absolutely essential that the temperature rises. No increased peristalsis of the bowels could be observed.

Treatment.

The treatment consisted in cold applications upon the abdomen; further, a subcutaneous infusion of 500 c.cm. of physiological salt solution and repeated collargol enemas, all without avail. Vomiting and singultus became uncontrollable on the following day, the transverse colon was strongly inflated, and on the evening of April 1st the entire abdomen distended to a great size, while the vomited matter possessed a faecal odour. At 10 p.m. I found the patient very much collapsed, the abdomen still inflated to a maximum, very sensitive to touch, while the patient vomited incessantly, groaning on account of the jerking pains, short breathing; pulse 120, temperature 37.5°; no flatus, in spite of high enemas.

Diagnosis and Result.

I now diagnosed the case as one of obturated ileus, caused by the retroverted uterus. I placed the patient in the knee-elbow position, alternated with abdominal position, and in the latter I ordered a high enema of camomile. The result was that abundant flatus was soon expelled, while the patient immediately felt relief. I left her about midnight, and she thanked me smilingly for the relief. The epigastrium yielded to pressure. Vomiting and singultus had disappeared entirely since the knee-elbow position.

Upon my advice the family physician injected during the same night 0.001 atropine, which was repeated twice during the following day. On three further days 500 c.cm. of physiological salt solution was injected subcutaneously, while high enemas were continued. During the critical night the pulse already became normal, while the temperature was normal during the entire attack. On the tenth day of childbed a gratifying stool was induced by castor oil; several oil enemas were given later. From the fourteenth day of childbed patient was able to feed her baby. Good recovery. In regard to differential diagnosis the relatively low pulse spoke against peritonitis; the normal temperature stood in strong contrast to the uncontrollable vomiting, singultus, and severe colics.

THE CAUSAL TREATMENT OF DYSTOCIA IN CASES OF PELVIC CONTRACTION.

By OTTO VON HERFF, M.D.,
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THE degree of dystocia in cases of pelvic contraction is essentially determined by three principal factors: First, the extent and character of the contraction; secondly, the size and compressibility of the fetal head; thirdly, the force of the labour pains. The principal factors are sometimes accompanied by others of a secondary and more accidental nature, such as are caused by malpresentations and faulty positions. Such cases demand special treatment.

A causal treatment of dystocia occasioned by pelvic contraction must choose as the object of its attack one or more of the above-mentioned factors. The third factor—namely, the force of the labour—is the factor we are the least able to influence with the remedies at our command.

Pelvic dystocia can therefore only be causally treated by dilating the pelves or by reducing the size of the fetal head, and, if possible, simultaneously enhancing its configurative capacity. Dilatation of the pelves, attained either by placing the patient in a special position or by division of the pelves, obviates pelvic dystocia without, as a rule, endangering the life of the child. By employing a special posture—for instance, the Walcher-Klein hanging posture for dystocia caused by a narrow superior strait, or the forced lithotomy position for dystocia due to a narrow inferior strait—dilatation can be produced, but unfortunately only in a slight degree. Still, even within such narrow limits these methods produce excellent results. They are disagreeable for the patient, it is true, but accompanied by no danger whatever for the mother and the child. The employment of these two postures as a causal treatment is so natural that it is superfluous to expatiate on them.

The various forms of pelvic section dilate the pelves in all their dimensions, but they jeopardize the life of the patient in no small degree. The mortality of mothers, at all times considerable, is increased by symphysiotomy to 8 per cent., by subcutaneous hebosteotomy to 4 per cent., and probably by subcutaneous symphysiotomy also to 4 per cent. The latter has, however, not yet been sufficiently employed to obtain definite statistics. Moreover, the lying-in period is by no means without its dangers. Often enough it is attended by complications, and unfortunately numerous injuries, more or less grave, often affect the health of the mother permanently. Amongst other unpleasant effects, we may mention as some of the most important an injury to the locomotive powers, bone-suture hernias, prolapsus of the vagina and the uterus, and last, but not least, the incontinence of urine. Should not one single case of this execrable complaint discredit the operation? There is no doubt but what the mother suffers disproportionately if the pelves are divided; at the same time 8 to 9 per cent. of the children are lost. Under these circumstances pelvic section, as a causal treatment, can only be valued as a last resource, when the mother absolutely desires a living child, and when the asepticism of the case is too doubtful to admit of any other form of delivery. Division of the pelves certainly has one advantage—namely, that a special posture, for instance, the Walcher-Klein hanging posture, can be employed as a preliminary procedure, and subsequent events left to time and Nature. In the event of immediate danger to mother and child, the operation can be made. This advantage cannot, however, make up for the danger to the life of the mother and the disproportionate loss of children. Pelvic section should only be employed as a last resource for multipara.

A further causal treatment of pelvic dystocia makes the child the object of attack. This can be done by the induction of premature labour about four weeks before the term, never by any means earlier, rather later, or by reducing the size of the child by an obstetric operation.

The diminution of the fetal head must ensue in all cases in which the life of the child is extinct. On this point all are unanimous. It is another thing if the child lives. Theoretically, and in all well-managed clinics, the sacrifice of a living child should be a thing of the past. As a fact, I have never in my life sacrificed a living child, except in the case of hydrocephalus. Still I must own that this rigid standpoint cannot be absolutely maintained in general practice, not even in clinics. Such extreme procedure should, however, be reduced to a minimum, at any rate, much more strictly avoided than it is at present.

By means of inducing premature labour a smaller head circumference can easily be attained, the head being at this period more compressible. This fact cannot be denied. In 80 to 90 per cent of such cases delivery occurs spontaneously. It is only necessary to operate in exceptional cases—for instance, in the case of a faulty position, or unfavourable presentation, or if some mistake has been made in calculating the obstruction. I have had to perform such secondary operations in 13.5 per cent. of 120 cases of induced labour. Puncturing the bag of membranes to induce premature labour is in itself such a simple affair that I should be ashamed to speak of it as an operation. For the same reason subsequent spontaneous delivery must be fully valued as such.[1]

The object of this causal treatment of pelvic dystocia is the child; the child is therefore liable to injury, the same as the mother is when pelvic section is performed. And, in fact, of the above-mentioned 120 children I was not able to dismiss more than 80 per cent. alive in the second week after delivery. Yet the chances of life of these children are in no way less favourable than those of children delivered by Caesarean section or symphysiotomy. In spite of this, we hear on all sides that children delivered by means of induced premature labour have slighter chances of life than others, and that, in consequence, this mode of causal treatment is of inferior value. One might feel inclined to present all who still believe in this myth with a thaler.

Even if 20 per cent. of the children born in premature labour die, which is about 11 to 12 per cent. more than in the case of pelvic section, we must not forget that the mother is hardly inconvenienced. Not only are the trials of a painful delivery considerably diminished, if not completely removed, but the lying-in period passes without any complication, the same as after delivery at full term. Neither is there any danger of a permanent injury to the patient, in strong contrast to the results of pelvic section. As regards mortality, it is *nil*, as in itself puncturing the membranes does not in the least endanger the life of the mother. Deaths occur as they do after other births, subsequent on haemorrhage or infection. It is true the latter should never occur. I myself lost but 1 of my 120 patients before mentioned, who died of haemorrhage after delivery, making a death-rate of 0.9 per cent. On consulting literature we find 0.6 per cent. quoted as the total proportion of deaths by infection, a number which is certainly too high for the present day.

Undoubtedly the causal treatment of pelvic dystocia with the induction of premature labour is somewhat more than twice as dangerous for the child as division of the pelves, but the mother is spared in every way. To deny this would be to aim a blow at truth.

One disadvantage of premature labour must certainly be mentioned—namely, that in some cases spontaneous delivery might have occurred at the end of the full term. Still there is no harm done; for such patients delivery is simply made easier; they enjoy, in fact, an advantage from the standpoint of humanity. The children are also less likely to suffer injury.

Theoretically the two forms of causal treatment can be combined, but the only practical and recommendable combination is with pelvic dilatation by means of the Walcher-Klein hanging position. Otherwise there is too much risk for mother and child.

A causal treatment cannot succeed if the dystocia is too severe; if a living child is the object it cannot even be recommended. All such cases necessitate a delivery which goes out of the way of obstructions—namely, the Caesarean section, subject as it is to the many modifications of fashion. By avoiding the obstructions the child is less endangered; its chances of life are excellent. The total losses amounted to 7.5 per cent., in 150 specially-chosen *Schauta* cases to 0 per cent. The mother, however, is exposed to considerable danger. The total death-rate shows 7 per cent., and even a strictly impartial choice of, for instance, *Schauta* cases shows 2.6 per cent. Moreover, the lying-in period is more troublesome and often accompanied by unlooked-for

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TABLE I.

Author.	Number of Contracted Pelves.	Per-centage.	Spontaneous Labour.	High Forceps and Premature Labour.	Caesarean Section and Division of the Pelves.	Maternal Mortality.		Infant Mortality.		Remarks.
						Total.	Reduced.	Total.	Reduced.	
DÖDERLEIN (Tübingen) (3,375 births)	759	Per Cent 24	Per Cent. 80	Per Cent. 2.3	Per Cent. 6.9	Per Cent. 0.5	Per Cent. 0.1	Per Cent. 11.2	Per Cent. 6.7	Mother died: 1 = Tuberculosis 1 = Gangraena cutis et pyaemia 1 = Peritonitis, infected at home 1 = Peritonitis
v. HERFF (Basle) (1902-1909) (10,289 births)	1425 Without 150 children below 2,500 grams 1275	13.8	79.5 With spontaneous labour after induced premature labour 86.2	10.4 Only with operations during premature labour 3.7	1.5	0.4	0	7.3	5.7	Mother died: 1 = Pyelonephritis calculosa 1 = Carcin. ventriculi 1 = Myodegeneratio cordis (kyphoscoliosis) 1 = Ruptura uteri, at home 1 = Atonia uteri 1 = Bacteriaemia, peritonitis, infected at home

Remarks.—(a) The statistics of Tübingen do not include 14 cases of placenta praevia, eclampsia, nephritis and phthisis with 13 deaths of children, all irregular contracted pelves and such with a conjugata vera measuring less than 6.5 cm. — number? —, all macerated fetus and children below 2,500 grams. (b) In the statistics of Basle all contracted pelves are included without any exception; all children below 2,500 grams are not included.

TABLE II.

Author.	Spontaneous Labour.	Operative Labour.	Forceps.		Podalic Version.		Premature Labour.	Breech and Footling Presentation.	Craniotomy Decapitation.		Division of the Pelves, Symphysiotomy, Pubiotomy.	Caesarean Section.	High Forceps, Prophylactic Version, Premature Labour.	Division of the Pelves, Caesarean Section.
			High.	Low.	Indicated.	Prophylactic.			Child Dead.	Child Alive.				
	Per Cent.	Per Cent.	Per Cent	Per Cent.	Per Cent	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.
DÖDERLEIN (Tübingen)	80	20	1.0	3.8	(1) 3.2	0.6	0.5	1.1	1.4	0.6	2.1	4	2.3	6.9
v. HERFF (Basle) 1902-1909	79.5 resp. 86.2	20.5 13.8	1.8	3.1	(2) 3.1	0	8.7	3.0	1.2	0	0.6	0.9	10.4 resp. 3.7	1.5

Podalic version in cases of transverse presentations: In Tübingen 13, in Basle 27 = 1.8 per cent. Version in cases of prolapsus of the umbilical cord or extremities: In Tübingen 12, in Basle 16=1.2 per cent. Version in cases of abnormal head presentations: In Basle, 5=0.3 per cent.; Tübingen? Breech and footling presentations of children of more than 2,500 grams = 28, and 17 of children below 2,500 grams = 45.

complications. Another thing to be feared is rupture of the uterus at a future birth, not to speak of cicatricial hernias. It is true the latter can be avoided by employing suprasymphysial section.

This summary shows that the accoucheur who, as I do, places the mother first must employ artificial premature labour as the causal treatment of pelvic dystocia. Those who place more weight on the life of the child must employ in the first place the Caesarean section, in the second place pelvic section. The choice of methods depends essentially on the personal value placed on the life of the mother; it does not depend on the results, of which I am now about to speak.

Numerous forms of pelvic dystocia permit a spontaneous delivery. This fact is not by any means new, as is often trumpeted abroad, but has always been known and taught. The general aim in treating pelvic dystocia should therefore be, in the first place, the promotion of spontaneous delivery; causal treatment must be reserved for graver cases. Nowadays the Parisian schools preach the strictest and most radical conservatism, as if it were an eminent achievement of modern surgery. According to them, we are to await delivery with eventual recourse to the Caesarean section, and to exclude the induction of premature labour, the use of the high forceps, and prophylactic version. As an example of the success of this method they quote Döderlein's results in Tübingen with 80 per cent. spontaneous deliveries, the mortality of mothers 0.5 to 0.1 per cent., that of children as 11.2 to 6.7 per cent.

We cannot but own that these are good results. But the fact is continually overlooked that the same results are to be obtained with less danger to the health of the mother by means of a moderate conservatism in the form of induced premature labour, and the use of the high forceps, leaving on one side prophylactic version. This has been sufficiently proved by the results of the hospital for women in Basle.[2] And just the fact that these results have been ignored induces me to appear before your forum with new material in the hope that my new statistics may receive more attention.

A glance through the above tables will show that a moderate conservative treatment of pelvic contraction by means of induced premature labour is productive of at least as good results as a radical conservatism combined with division of the pelves and Caesarean section. It will also prove that it is very easy to obtain a large number of spontaneous deliveries by means of premature labour. Basle, for instance, can show 87.6 per cent., Tübingen only 80 per cent. At the same time it entails much less danger to the life of the mother. The risk to the child is about the same.

There can be no doubt that accoucheurs who estimate the life of the mother higher than that of the child, who sympathize more deeply with all who suffer in consequence of contracted pelves, must certainly employ the induction of premature labour, a method so easily carried out by means of puncturing the membranes.

I sincerely hope that this very salutary mode of procedure, of which English science can be proud, may also in future prove a blessing to many, and rise again some day to a place of honour in the medical world.

REFERENCES.

1 *Muench. med. Woch*, 1908, No. 50. *Volkmann's klin. Vorträge*, N.F., No. 386 (Gynäkologie, No. 143), January, 1905. 2 *Volkmann's klin. Vorträge*, N.F., No. 487 (Gynäkologie, No. 177), June, 1908. *Monats. f. Geb. u. Gyn.*, Bd. xxiv, p. 16. Liebermann, Dissertation, Basle, 1908.

DISCUSSION.

Dr. TATE (London) agreed with Professor von Herff in his strong advocacy of the induction of premature labour when a moderate degree of pelvic contraction was present and there was a reasonable chance of delivering a living child. Caesarean section and other surgical procedures for enlarging the pelvis should not be undertaken unless the induction of premature labour had ended in failure to deliver a viable child on previous occasions.

Dr. WALTER SWAYNE (Bristol), after drawing attention to the influence of locality on the number of cases of dystocia due to contracted pelvis and the marked difference exhibited by the statistics of various large towns in this respect, referred to variations in the number of the severer operations in different localities due to local conditions. He considered that undoubtedly there had been of late a tendency to resort to the severer operative measures, such as Caesarean section and symphysiotomy, rather too freely, and that in some cases the former operation had been done in the absence of the "absolute indications" of marked pelvic contraction. He was of opinion that the narrow limits between which the operations for widening the pelvis (for example, symphysiotomy and pubiotomy) were admissible, and the fact that these operations did not in all cases lead to the result hoped for militated strongly against their more general adoption. He rather welcomed von Herff's advocacy of the less drastic methods set forth in his paper.

Dr. R C. BUIST (Dundee) said that if the indications for division of the pelvis were the desire of the mother for a living child or doubtful asepsis, the verdict on the operation would be unfavourable. In cases of moderate pelvic contraction Prochownik's method of restricted diet was a valuable adjunct. If premature labour was to be induced, puncture of the membranes was the method of election.

The PRESIDENT (Dr. Mary Scharlieb) said that in minor degrees of pelvic deformity the induction of premature labour was a valuable treatment. As soon as it was evident that the child's head had reached the size at which it was a tight fit for the pelvis, labour should be induced. The necessary condition was that the child should be not only viable, but so far developed that it was likely to survive. The operations of symphysiotomy and pubiotomy had no doubt a place in modern midwifery, but they were only advisable for a skilled practitioner and under favourable circumstances. Removal of the child at full term per abdomen had become a safe and successful operation since aseptic surgery and rapidity of operation had reduced the maternal mortality to 5 per cent. or thereabouts. She could not agree with Dr. Buist that treatment by restricted diet was likely to reduce the size of the child. During famines in India emaciated and even dying women gave birth to well-nourished children.

FIBROID TUMOURS OF THE UTERUS AS A COMPLICATION OF PREGNANCY.

By CUTHBERT LOCKYER, M.D., F.R.C.S.,

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THE subject of this paper is based on the experience gained in the treatment of fibroids of the uterus observed during pregnancy. The details of 9 cases are recorded. For 4 of these operation was performed; in 2 cases this consisted in the removal of the entire uterus in the gravid state together with the tumour, in one instance retro-peritoneal hysterectomy was carried out, whilst in the fourth myomectomy with conservation of the uterus was performed, the uterus subsequently bearing the child to term, when a normal labour and puerperium followed.

Of the 5 cases for which no operation was deemed necessary, 3 were delivered of full-term children and did well in the puerperium and afterwards. One is still pregnant and is being watched, and the fifth aborted; in this case it was necessary to remove the placenta piecemeal, an operation which proved to be very difficult. From a careful survey of the literature on this subject and from my own experience, I have drawn the conclusion that in the majority of cases where fibroids coexist in the gravid uterus no operative interference is called for; therefore my own experience is somewhat unusual, inasmuch as in 4 of the 9 cases recorded there *was* urgent need of operation. In one instance the urgency of the case was in no measure due to the fibroid but to the coexistence of a large ovarian cyst, and if the latter had not existed the fibroid might have been left alone, as it was situated at the fundus, where it would have caused no trouble; moreover, its size was inconsiderable. The above remarks apply to Case III, in which myomectomy was performed. This case is only of interest here as showing that the gravid uterus need not be disturbed by the enucleation of a subperitoneal fibroid.

In Cases I, II, and IV the fibroids were incarcerated in the pelvis. In Case I the fibroid was of the typical cervical type, and was embedded in the posterior lip, and furious haemorrhage had led to a missed abortion.

In Case II I had hoped to save the case for Caesarean section, but severe bleeding necessitated immediate interference, and it was found that incomplete abortion had occurred before operation, as only decidual tissue was discovered in the enlarged cavity at the time of hysterectomy. In Case IV (Mr. Meredith's case), as the illustration shows, the tumour was of the burrowing type. It lay partly in the right broad ligament, and had pushed its way behind the bladder, the cervix lying at a higher level than the pubes. Retention of urine, loss of flesh, and constipation were the indications which justified Mr. Meredith for operating before the child was viable.

Whilst fully realizing that in most instances fibroids may safely be left alone when met with in pregnant women, Cases I, II, and IV show that there are exceptions where the risks of non-interference are great. These risks are well known to gynaecologists, for so long ago as 1901 Dr. Archibald Donald read a very able paper before the Obstetrical Society of London, pointing out that these risks may be summed up as follows:

1. Rapid increase in size of the tumour causing severe pain and great distress.
2. Incarceration of the tumour in the pelvis.
3. Serious pressure on the bladder.
4. Degeneration of the tumour through diminished nutrition.
5. Excessive rotation of the pregnant uterus.
6. Abortion or premature labour as a result of pressure or of submucous development of the fibroid.

In my cases incarceration of the tumour occurred in Cases I and II; in Case IV "severe pain and great distress," with "retention of urine," were the complications; whilst abortion occurred in Cases II and VIII.

Due regard should be taken of the situation of the tumour when deciding what course is to be pursued in the treatment of the case. If the fibroid lies above the pelvic brim, as it did in my cases No. III and No. VII, it is not likely to give rise to trouble. If it lies in the pelvis and cannot be pushed up, the case should be watched in the hope that the pregnancy will continue to term, when Caesarean section followed by hysterectomy should be performed. If any of the above risks supervene and the patient's wellbeing becomes imperilled, immediate interference is called for. The nature of such interference will depend upon the character of the growth. Myomectomy, wherever possible, should be tried; if the tumour is localized and not embedded too deeply in the uterine wall, and if the bleeding can be effectually stopped, this operation has proved successful in allowing pregnancy to continue. This is proved in Case III of this small series, whilst Cullen, Bland-Sutton, Walls, and Donald have recorded like successes. It is, however, highly probable that such cases are comparatively rare, and more often hysterectomy would be the safer operation. In no instance can I conceive it right to induce abortion in cases of fibromyomata of the uterus complicating pregnancy. The details of the 9 cases above referred to are as follows:

Four Cases of Fibroid Tumours complicating Pregnancy for which Operation was Performed.

CASE I.

F. C., aged 33 years, four children and one miscarriage, was sent to my out-patient clinic by Dr. Cardy Bluck, with a note saying that the patient had miscarried, but that the uterus was large and required curetting for persistent haemorrhage which had continued for three weeks.

On admission into the Great Northern Hospital on January 20th, 1910, the patient complained of pelvic pain, but the haemorrhage had ceased. She stated that five weeks ago she had a very severe flooding which ended in an abortion at the second month of gestation. She was very weak and ill, and remained in bed for three weeks, losing blood the whole time. The bleeding gradually stopped, and for the past fortnight had not recurred.

The patient had been treated for haemoptysis in the Brompton Hospital on two occasions—once in 1905, when she remained in hospital for three months, and again in 1906, when she stayed in for a similar period.

Examination on Admission.—Cervix uteri is low down and pushed to the left side by a pelvic tumour which lies in front and to the right, and which has expanded the portio and drawn it up. The cavity of the uterus is eccentric, peing pushed to the left by the tumour; the sound passes 5½ in. There is a soft cystic tumour lying high up in the iliac fossa on the left side. It is freely movable, as if pedunculated, but does not influence the pelvic tumour by its movements, the latter being fixed. I made the diagnosis of fibromyoma uteri occupying the pelvis, and considered that the abdominal tumour was an ovarian cyst, owing to its semicystic nature, and to the fact that it seemed pedunculated and detached from the fixed pelvic mass. The indications for operation were great pelvic pain extending over a period of five weeks, with pressure symptoms relative to bladder and rectum.

Operation.—On January 29th, 1910, I opened the abdomen, and found that the abdominal tumour was the gravid uterus, whilst the pelvic mass was a typical cervical fibroid. Panhysterectomy was performed. The fibroid had completely drawn up the lips of the cervix, and had commenced to open up the external os, through which it was already bulging. The growth occupied the posterior wall of the cervix, so that the cervical canal lay in front of it. The appendages on both sides were removed. On stripping down the peritoneum with the vessels on the left side there was furious haemorrhage for a few seconds issuing from the uterus itself (backward flow). The cervical fibroid shelled out easily from the connective tissue of the parametrium. Much time was spent in suturing the lateral angles of the amputated vagina. The abdominal wound was sewn in four layers, and two interrupted sutures were inserted to close the cavum Retzii. The operation occupied fifty-five minutes, including the abdominal suturing. The patient stood the ordeal well, and made an uninterrupted recovery.

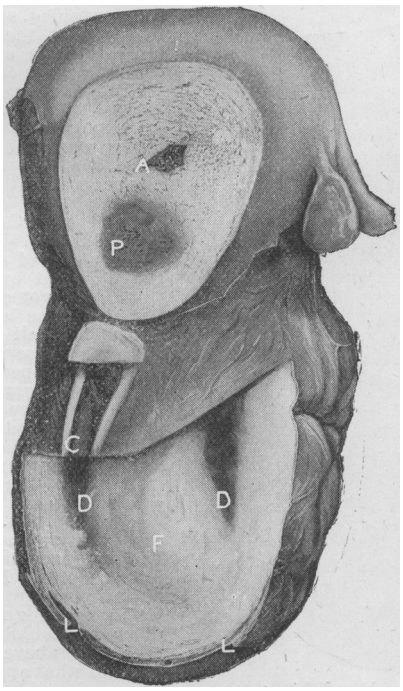

Fig. 1.—Case I. Cervical fibroid beneath a gravid uterus. Seen from the back. A, amniotic cavity; P, placental attachment; C, cervix; F, fibroid; L L, lips of cervix; D D, areas of degeneration.

The cervical fibroid shows on section a wide horseshoe area of black (not red) degeneration (vide Figs. 1 and 2).

Case II.

C. A. was sent to me by Dr. Hardy, of Barnet, for abdominal tumour complicating pregnancy. I first saw her at the Great Northern Hospital on January 26th, 1910, when she gave the following history: Aged 39 years, married twelve years, one child born dead eleven years ago, no miscarriage. Menstruation started at the age of 13 years, always regular, always profuse, with the passage of clots. Menstruation ceased on November 29th, 1909, and the patient considered herself two months pregnant. She had consulted Dr. Hardy for a painful lump in the right side of the pelvis above Poupart's ligament, which was first observed in November, 1909. On examination the uterus was much enlarged, and projecting from it was a lump the size of a hen's egg, which lay above the pelvic brim on the right side. The size of the uterus did not correspond with the dates of gestation, and this was attributed to another fibroid lobe lying in Douglas's pouch; the body of the uterus could not be made out apart from the fibroid mass. I wrote to Dr. Hardy agreeing with his diagnosis, and advising that the case be watched, explaining that I could not raise the pelvic mass, and that it might be necessary to deliver the child at term by abdominal Caesarean section.

Quite suddenly, however, the patient was taken with severe uterine pains and profuse haemorrhage, and thirteen days later (February 8th, 1910) had to be admitted for an alarming flooding. On examination in the ward the cervix was found to be patulous and soft; the mass in Douglas's pouch extended into both lateral fornices; it was still impossible to make out the body of the uterus apart from it.

On February 9th, 1910, I removed the uterus by supravaginal retroperitoneal hysterectomy after the method of Howard Kelly. On opening the cavity of the uterus it was found to be empty, excepting for some adherent shreds of tissue in the left cornu. These on examination proved to be bits of decidual tissue; the embryo was never seen, but must have escaped in the severe haemorrhage from which the patient suffered a day or two before admission. The patient made a smooth recovery.

Case III.

Mrs. X. was sent to me by Dr. Hodges, of Watton-at-Stone, on November 27th, 1909, on account of difficulty in micturition, which almost amounted to retention, very small quantities of urine being passed at a time. The lady was 40 years of age; she had missed two periods, and considered herself pregnant. She had been married fifteen years, had had four children, the youngest being 6 years of age. Constipation was complained of, and there had been pain in the left iliac fossa for fourteen days. On examination a prominent oval swelling could be felt in the left iliac fossa, with a hard lump projecting on its right (mesial) side above the otherwise smooth surface of the tumour. Dr. Amand Routh kindly saw the case with me, and we discovered in addition to the above that there was a cystic growth in the posterior part of the pelvis, which pushed up the cervix under the symphysis pubis and pressed strongly upon the rectum posteriorly. This tumour could be pushed up into the pelvic brim and above it to the right of the uterus; but whilst in this position a portion of the cyst still lay in the pouch of Douglas, which it depressed below the level of the cervix. Dr. Routh agreed that this was a cyst of the right ovary, and that the tumour lying above the pelvic brim high up in the iliac fossa and lumbar region was the uterus with a fibroid at its right cornu. On November 30th I removed a right-sided ovarian cyst with papillomata projecting through its wall, and also a fibroid from the right cornu of the uterus. The fibroid shelled out easily, and its bed was sewn together without notable haemorrhage. Convalescence was uneventful, and the patient got up on December 15th and motored home next day. The confinement took place on July 13th, 1910. It was spontaneous, a female child being born before Dr. Hodge's arrival at the patient's house. The puerperium is progressing satisfactorily.

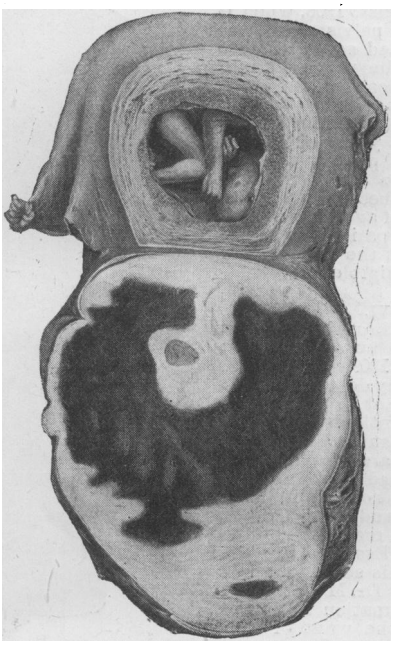

Fig. 2.—Case I. Cervical fibroid beneath a gravid uterus. Seen from the front. Note the dark area of black pigmentation ("red degeneration").

October, 1910.—The patient is now in perfect health.

I am indebted to my late colleague, Mr. W. A. Meredith, for his kind permission to publish the following case:

Case IV.

Mrs. McC., aged 39 years, married eight years, no children. Complaint: Pain and enlargement in the lower abdomen for four months; enlargement increasing rapidly. Retention of urine, loss of flesh, constipation, leucorrhoea. Mr. Meredith saw this lady in August, 1902, when she stated that the catamenia had been regular until May 8th, 1902, since when she had seen nothing. On examination there was a hard mobile nodular tumour reaching 8½ in. above the pubes; transversely it extended 6 in. to the right and 3½ in. to the left of the midline at the level of the umbilicus; there was also an elastic fluctuating swelling in Douglas's pouch continuous with the abdominal swelling. The cervix was high up in front above the pubes.

Mr. Meredith diagnosed fibromyoma complicating pregnancy. The need for immediate operation was obvious. The fluctuating pelvic mass proved to be the pregnant uterus, from the upper

and front part of which grew a sessile multiple fibromyoma. The anterior and right-sided portions of the tumour had burrowed behind the bladder and into the right broad ligament. The ovaries were not removed. In this operation Mr. Meredith was assisted by Mr. Corrie Keep. The patient made a good recovery, and in a letter from Mr. Meredith he tells me she is quite well at the present time—that is, eight years after the operation.

The specimen is illustrated in Fig. 3.

Five Cases of Fibroid Tumours complicating Pregnancy for which no Operation was Performed.

CASE V.

S. M. was sent to me by Dr. Ward Clarke for pelvic tumour. I first saw her in August, 1904, and found a fibroid in the pouch of Douglas, which appeared to be pedunculated and attached to the posterior wall of the uterus. As this tumour was only the size of a hen's egg, and had caused no haemorrhage nor pressure symptoms, I advised against interference. Three months later the patient married (November, 1904), and I heard nothing of her until February, 1906, when she suspected that she was pregnant. The last period began on December 1st, 1905, and lasted one week. Sickness, starting in the morning and, lasting all day, set in after the period ended, and continued all through January, when she came to the Samaritan Hospital. On examination I discovered two fibroids in connexion with a bulky uterus, about the size of an eight weeks gestation. One fibroid lay in Douglas's pouch, from which it could not be pushed up, and another lay to the left of the uterus higher up. The patient was admitted in August, 1906, when it was found that the pelvis was quite free, the growths had ascended into the abdomen, and she was sent into Queen Charlotte's Hospital, where a normal confinement took place a month later (September, 1906). The child, now nearly 3 years of age, is a healthy girl, the patient is well, the fibroids cause no symptoms, but she has never again become pregnant, and is now 42 years of age.

I examined this patient in July, 1910, and found that the fibroids had shrunk to a quite negligible size.

CASE VI.

Mrs. W. O. B. was brought to me by her husband without an appointment on February 25th, 1909. Mr. O. B. stated that the family doctor had sent his wife to a consultant, and that they had just left this gentleman very dissatisfied as to the result of the interview, and wished for a second opinion. The husband explained that his wife was formerly a patient of Dr. Mailer's, of New Southgate, and that he had wished her to consult me before taking up her residence in Essex, where she lived at present. This statement was afterwards verified by Dr. Mailer.

I obtained no information as to the cause or nature of the want of confidence in the former consultant until after giving my opinion in the case. The patient was aged 34 years; she had been married twelve years; there was one child aged 9, and a miscarriage occurred four years ago. There had been amenorrhoea since November, 1908, and the patient had been ill in bed for the past six weeks; the family doctor had examined and told the patient she had a fibroid tumour and was pregnant.

I found her looking very thin and ill; the uterus was the size of a three and a half months gestation; there was fluid in the breasts and a fibroid in the anterior wall of the isthmus of the uterus. I advised the patient not to worry about the growth, saying that I thought it would move out of the way as pregnancy advanced, and it was then that I learnt from the husband that consultant No. 1 had urged an immediate operation. My advice was more acceptable, and the patient was safely delivered by forceps of a strong, healthy female child on August 29th, 1909. Dr. Smellie, of Westcliff-on-Sea, by whom the patient was delivered, wrote to me to say: "The fibroid gave no trouble, but the patient was in a very indifferent state of health at the time, and I had to use forceps when she had been in labour about eight hours. It was a low forceps case, and everything went well until I was just about to leave, when the patient became sick and almost pulseless, but there never was any bleeding. The pulse, however, remained rapid for twenty-four hours, yet there was no obvious reason for the collapse, and the husband says the same thing happened with the first child nine years ago."

This patient is now in good health and has not again become pregnant.

CASE VII.

On March 7th, 1910, Dr. Duncan, of Richmond, asked me to see a case of pregnancy with an extrauterine swelling which caused him some anxiety. The patient was aged 33 years, married thirteen months; had had an abortion in August, 1909. Menstruation was regular every thirty days, lasting for five days. Two or three towels were used daily; the last menstruation dated from the 23rd to the 28th October, 1909, and the patient dated her conception from November 16th. Morning sickness started on November 30th. There had been increased frequency of micturition since February, 1910. On March 1st sharp pain was felt in the right iliac fossa accompanied by sickness. The pain kept the patient awake all night, and was accompanied by great tenderness in the right iliac fossa. The pain came in attacks, and three such were experienced on March 2nd. On March 3rd, when Dr. Duncan saw the patient, she told him she was four months pregnant, and said that when the pain and tenderness started on the right side her attention was first drawn to a swelling there, which she had not noticed before; the swelling caused extreme pain on movement in bed. The abdomen was rigid, but moved freely on respiration. Dr. Duncan noted a swelling the size of a fetal head above Poupart's ligament and extending under the right rectus muscle towards the mid-line. The fundus was at the umbilical level. The swelling was extremely tender, and rendered examination difficult. The temperature was normal. On examination under anaesthesia on March 7th, I found a gravid uterus reaching to the navel, and somewhat deflected to the left by a solid, hard growth projecting from its right wall just above the pelvic brim, but not in any way encroaching on the pelvis; the vaginal vaults were empty, and there was in fact nothing abnormal in the pelvis, and the latter was not contracted. I diagnosed the presence of a fibroid and advised leaving it alone. The pain soon subsided, and Dr. Duncan writes to say that the patient is at present (July 12th) in excellent health. The confinement is due in August next.

N.B.—I have since heard from Dr. Duncan that he delivered the patient by forceps of a full-term child. The labour was difficult, as the child was a large one, but the puerperium ran a normal course.

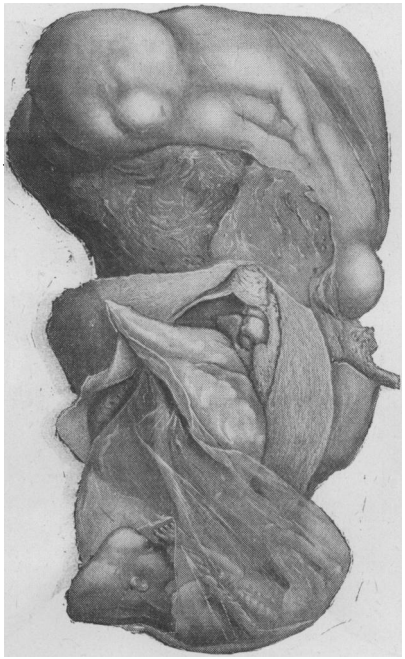

Fig. 3.—Case IV Mr. Meredith's case of total hysterectomy for fibroid complicating pregnancy. The raw area, devoid of peritoneum, shows the extent of burrowing.

CASE VIII.

In 1903 Dr. Victor Sapp asked me to see a case of severe uterine haemorrhage caused by an incomplete abortion. The woman was a primipara aged 30 years. She had been pregnant for two and a half months when she was seized by a severe flooding in which the fetus escaped; the patient was put to bed and ergot was administered, but the bleeding continued. On arrival I found the patient blanched from loss of blood, but well nourished and very muscular. The uterus was well above the pubes; in fact, its size was equal to that of a sixteen weeks gestation. The os was patulous and the vagina full of blood clot. Under anaesthesia, administered by Mr. Anderson, I found a large interstitial fibroid growth in the right uterine wall projecting into the cavity of the uterus. I could feel the placental mass high up above the fibroid, tucked away in what appeared to be the right cornu. I could not get my finger above the mass, and digital separation was quite impossible. Accordingly I used ovum forceps with a view to delivering the placenta piecemeal, but at the conclusion of the operation I was quite uncertain as to whether the entire structure had been removed, as I could not get my finger around the growth and up into the recess in which the placenta was situated. I flushed the cavity out with a solution of iodine through a flushing curette, and packed the uterus with iodoform gauze soaked in iodine pigment (1 to 4), and the patient made a good recovery. I mention this case as an illustration of the difficulty of removing the placenta in some cases of abortion occurring in cases of fibroids of the uterus.

CASE IX.

Mrs. H., sent to me by Dr. Fuller, of Crouch End, for fibroids of the uterus, with possible pregnancy. The patient was 38 years of age, had been married for fourteen years; sterile. The patient was seen by me in July, 1910; she complained of pain in the left iliac region, and a sense of bearing down. She had rested in bed for one week on account of the pain, and the latter had abated completely. The periods had always been quite regular and never profuse, but always painful. The last menstrual epoch occurred in March, 1910, since when there had been no loss whatever. There was a certain amount of leucorrhoea and chronic constipation, but no urinary symptoms beyond slight increased frequency, and no piles. On examination the patient was well nourished. There was fluid in the breasts, a greasy semilune at the base of the nipples, and general mammary congestion. At the umbilical level there was a rounded fibroid proceeding from the fundus uteri about the size of an orange, and freely movable. The fundus reached halfway up to the navel, and growing from the anterior uterine

wall were two other fibroid growths about the size of golf balls. Behind, in the pouch of Douglas, was another growth, starting apparently above the isthmus, and which ascended with pressure, and therefore led to the belief that it would not give rise to obstruction in delivery. I counselled leaving the case to go to term, in the belief that pregnancy, labour, and involution would not be influenced adversely by the four growths.

PAPILLARY OVARIAN CYSTS: SHOULD BOTH OVARIES BE REMOVED?

By Professor W. NAGEL,
Berlin.

SINCE the investigations of Wilson Fox, Waldeyer, and Thornton, papillary tumours have taken a special place among ovarian tumours; for papillary growths are signs of an increased cellular proliferation, which in the clinical sense always implies a certain stage of malignancy. To be sure papillary cystomata have since the above mentioned investigations received a different classification, though it is not yet quite clear whether they originate from the par.

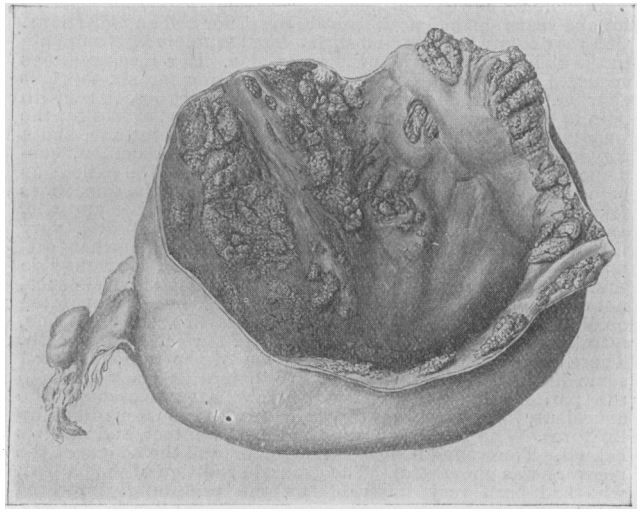

Fig. 1.

ovarium, from the epithelium of the tubes, or from the germinal epithelium. Yet their anatomical and clinical appearance have remained the same. Almost the half of all papillary ovarian tumours are adeno-carcinomata; the others only represent a particular kind of cystadenoma,

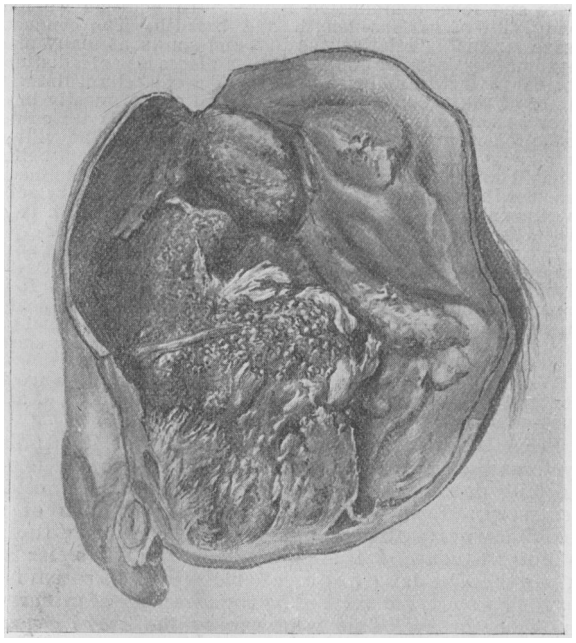

Fig. 2.

inasmuch as any group of cystadenoma may produce papillary growth. They are unilocular or multilocular cysts with more or less thin walls, and the formation of villi is found within the cysts as well as on the surface of the tumours. Sometimes the villi are spread diffusely, ex-

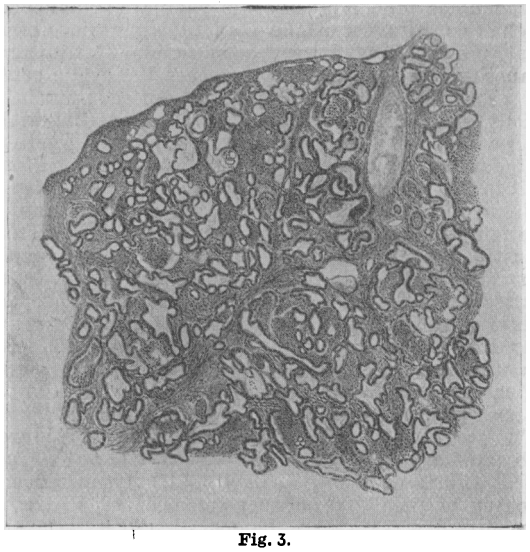

Fig. 3.

tending more or less over the inner surface of a larger cystic cavity (see Fig. 1); sometimes a more or less large

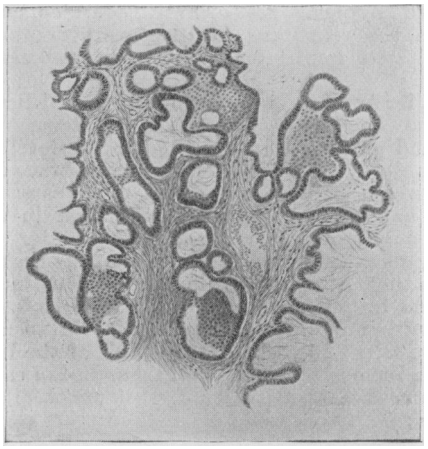

Fig. 4.

cyst is almost entirely filled by a very much ramified tree of villi, the amount of fluid being very small (see

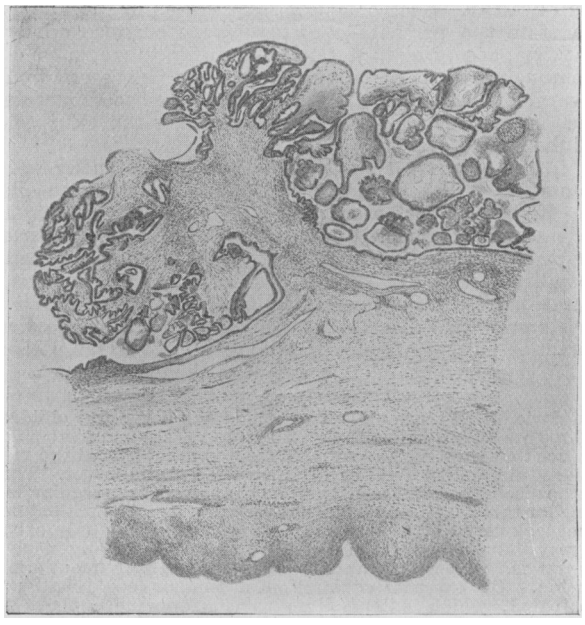

Fig. 5.

Fig. 2). The villi consist of a connective tissue substratum, rich in vessels, and covered by an epithelial layer. (Figs. 3, 4, 5).

The contents of the cyst are sometimes colloid, and sometimes serous. In one kind of papillary tumours (cystadenoma pseudo-mucinosum papillare (Pfannensteil)) the chief constituents of the contents are various kinds of pseudo-mucin secreted by the epithelium of the tumour. In another type of papillary tumours the cystic contents are serous, containing, however, no pseudo-mucin, but copious amounts of albumen. The internal wall of the papillae is mostly lined by ciliated epithelium (*cystadenoma serosum papillare*, Pfannensteil). Pfannensteil[1] in his classification has chiefly considered the composition of the cystic contents, yet it is still an open question whether his classification of papillary cystomata is justified histogenetically. The frequency of papillary cystomata, according to Schmidtlechner's[2] statistics of Tauffer's clinic, is 8 33 per cent. of all ovarian neoplasms, both ovaries being frequently involved at the same time. According to Tauffer, the papillary adenocystoma serosum was unilateral in 33 per cent., bilateral in 67 per cent of his cases. Glockner[3] (Zweifel's clinic, Leipzig) found bilateral tumours in 60 per cent. of all cases.

The prognosis after operation is not bad. Of 34 women with papillary cystoma, according to Höhne[4] (Gynaecological Clinic of Kiel), 18 have had no recurrence. Of 45 cases of papillary adenocystoma, 31, or 83 per cent., were alive five years after the operation (Tauffer).

According to these figures we certainly cannot deny that papillary cystoma frequently involves both ovaries at the same time. On the other hand, it seems to me that the alleged frequent post-operative development of a tumour in the remaining healthy ovary depends more upon subjective impressions than upon well-established facts.

I learn, for example, from Hofmeier's[5] statistics that of 8 unilateral operations for papilloma, 7 have remained healthy, and only in 1 case a tumour had developed one year after the operation. Tauffer[6] could trace 17 of his cases, of whom 12 lived more than five years after the operation, and the development of a tumour in the other ovary has only been observed in 2 cases.

These facts are less striking if we consider that even in carcinoma the post-operative affection of the ovary which is left in the body is not very frequent. Of Hofmeier's 30 patients with unilateral ovarian carcinoma 15 (= 50 per cent.) were alive; 15 died in the course of the first eight years after the operation, and 7 of them died of carcinoma. Tauffer traced 2 of 9 patients with unilateral ovarian carcinoma; both were well nine and ten years respectively after the operation.

Of Fromme's[7] 21 cases of unilateral ovarian carcinoma only in 2 cases had a unilateral ovariotomy been performed. One of them had died, but the other patient was still healthy. Such complete statistics as might allow us to state the percentage of permanent cures are indeed lacking; according to Hofmeier, however, to whom we owe thorough investigations, the tendency of the second intact ovary, left in the body at the operation, to become carcinomatous, is not great enough to indicate the extirpation of the second ovary.

In the discussion on the permanent cures after ovariotomy at the Eleventh Congress of German Gynaecologists in Kiel, 1905, some cases of pregnancy after unilateral operation for carcinoma, or papillary cystoma, were reported, namely: Hofmeier, 3 cases (2 after carcinoma, 1 after papillary cystoma); Tauffer, 3 cases (1 after carcinoma, 2 after papillary cystoma); Fromme, 1 case of papillary cystoma. To these I should like to add 2 cases of my practice. These are of a particular interest, as the patients have remained completely well after the operation.

CASE I.

Marie M., 37 years old. The patient has borne five children, three are alive and healthy. Two years ago she had an abortion. Since that time she had suffered from pain, chiefly at the right side. She thinks that her abdomen has become larger. There is no discharge. Her menstruation has always been regular, lasting for three days, and being not very profuse. The patient does not look very bad, and states that the tumour has only caused great distress during the last few months, especially in going upstairs. She complains of constipation and of frequent micturition. On external exploration a tumour could easily be palpated, which extended up to a handbreadth above the umbilicus. Bimanually the tumour can be defined from the anteverted uterus, showing a slight fluctuation.

I performed laparotomy April 5th, 1902. The tumour was of the size of a man's head, and there were no adhesions. After emptying the colloid contents by means of a trocar the tumour collapsed, and was easily removed in the usual way. The left ovary I left behind. Uncomplicated recovery took place.

The cystic wall is of a solid consistence, several millimetres thick. One cyst, the size of a walnut, was encapsulated in the chief cyst. The tumour was connected with the tube by the intact mesovarium, which gradually passed over into the surface of the cystoma. No trace of the ovary was to be found. On the internal surface the wall of the tumour was lined with copious papillary proliferations, not spread uniformly, but lying more on the side facing the hilus. Microscopically, the cystic wall exhibited the well-known structure. The internal layer consisted of the papillae proper, in some places so slender that it was difficult to recognize the connective tissue substratum, and in others forming nicely ramified villi; moreover, tubules were seen in cross section invaginated in the wall. The villi, as well as the tubules, were only present in a very small number, and did not extend very deeply into the stroma. After the operation the patient recovered very rapidly. In March, 1903, menstruation failed, and the patient had an easy and successful confinement on December 22nd. The child is doing well. I examined the patient for the last time on June 25th, 1910. The uterus was at that time in normal anteversion and not enlarged, left appendages normal, no resistance to be felt. She was in perfectly good health.

CASE II.

Auguste B., 30 years of age. The patient had been married for one year; she had not borne children, nor had any abortions. One year before the wedding she suffered temporarily from pain in the abdomen, mainly on the right side. Her menstruations occurred every third or fourth week, lasting for six to eight days, with profuse haemorrhage, and sometimes associated with pain in the loins. This continued for some time, even after the wedding, until the pains in the loin became intense, and a copious discharge ensued. The pain in the abdomen, continuous during the last months, was described by the patient as being like a girdle sensation—spasmodic—so that she sometimes fainted. The pain deprived her of sleep, she lost her appetite, and broke down in a short time. The patient appeared very miserable and anaemic. Her abdomen was somewhat distended. The virginal uterus was anteflexed. On the right side there was a scarcely movable tumour of an elastic consistence, not quite spheroid, and of the size of a child's head. I operated on March 10th, 1902. The tumour was located at the right side, intraligamentary and immovable. Its surface was smooth. There was no ascites. The enucleation of the tumour offered some difficulties, as it was closely adherent at many places to the peritoneum of Douglas's pouch. After loosening the adhesions, the tumour was separated from its surroundings by ligatures. The left ovary appeared to be intact, and was left behind. The wound healed per primam, and the course of the recovery was uneventful. The general condition of the patient, who had been very miserable at the beginning, improved gradually, and she was able to leave the clinic after four weeks. During the operation I had noticed the hardness of the tumour, and had therefore suspected a carcinoma. The tumour contained a small amount of colloid liquid. On its surface we found the above-mentioned adhesions, but no papillae. Its wall was from 1 to 3 mm. thick, and the unilocular cyst was almost entirely filled with papillary proliferations. In the microscopic picture the large amount of invaginated glandular tubules is especially noticeable; they are ramified here and there and form complicated figures. In addition we found many villi of various length and breadth. The connective tissue stroma is relatively scanty and shows at many places myxomatous degeneration. At other places it is of a distinctly fibrillary structure and rich in cells. The epithelium lining the lumen of the tubules is of a cylindrical shape, mostly in one layer, at some places in several layers. The cells are cubical, and project more or less into the stroma. The villi are uniformly lined with one layer of columnar epithelium. Ciliated epithelium was not to be found. One is inclined to consider the tumour as probably a carcinoma (papillary adenocarcinoma), although the original purely papillary form is still distinctly marked.

Two years after the operation the patient became pregnant for the first time, and was delivered by forceps of a girl on October 24th, 1904. I examined her again on March 20th, 1909. In the meantime she had borne two more children. The last confinement was in November, 1907. The uterus was in a normal position, not enlarged, and the left appendages were intact; she felt perfectly well.

(When inquiring about her present state a few days ago her husband wrote me that she died on April 28th, 1909, of acute pneumonia.)

From these observations I conclude that in papillary cystoma the extirpation of the other healthy ovary is not, as laid down by Pfannenstiel, Glockner, and others, necessary. Even in carcinoma similar affection of the remaining ovary after the removal of the primary tumour is not frequent. I therefore agree with Hofmeier and others that the danger of a possible second operation is in a young woman outweighed by the advantage of preserving the other ovary. This is proven by the above cases of pregnancy after unilateral operation.

REFERENCES.

[1] *Handbuch der Gynäkologie;* herausgegeben von F. Veit. [2] *Monatsschr. f. Geb. u. Gyn.* Bd. xxviii. [3] *Verh. der deutschen Ges. f. Gyn.*, Band x. [4] Ibid, Band xi. [5] Ibid., Band xi. [6] Ibid. [7] Ibid.

DISCUSSION.

Professor Gottschalk (Berlin) agreed that when removing a proliferating papillary ovarian cyst one might leave the other ovary if it appeared unchanged. He had operated twenty-one years ago upon a girl 23 years old on account of a large proliferating papillary ovarian cyst. He saw her again six months ago in good health, and she was now the mother of three children. Another case on which he operated eight years ago was still in good health. It was, however, necessary to keep the patient under strict observation, so as to operate without delay if the other ovary became enlarged. In cases in which it was doubtful whether the other ovary was sound he would recommend that the second ovary be split for the purpose of examination. If the ovary proved sound it could be stitched up with catgut and left in its place.

Professor Kynoch (Dundee) said that most gynaecologists must have seen malignancy occur in presumably healthy ovaries left after the removal of what appeared at the time of operation to be ordinary multilocular cysts. But those cases were so rare as not to warrant the removal of apparently healthy organs in multilocular cysts, as pregnancy might occur. But in papillary ovarian cysts, of which so many were malignant in character, the question of removal of the apparently healthy ovary deserved more consideration. He had had two cases in which cancer of the peritoneum and ovary followed four and seven years after removal of a unilocular papillary ovarian cyst. In patients under 40 or so he left the second ovary if it appeared healthy, but in patients at or about the menopause he always removed the apparently healthy ovary. All cases of removal of unilocular papillary cysts required to be more carefully observed than when only ordinary ovarian cyst was in question.

Dr. Munro Kerr (Glasgow) agreed with Dr. Nagel that if the papillary ovarian cyst seemed to be not very malignant, then the other ovary should be left if it were apparently absolutely healthy. But on the other hand, if the tumour appeared to be malignant (of course this was often difficult and frequently impossible to determine), then the other ovary should be removed, and not only the ovary but the whole uterus should be extirpated. At present the generally accepted operative procedure in malignant disease was to remove as much surrounding tissue as possible. Surely, therefore, in malignant disease of the ovary the whole of the internal reproductive organs should be removed.

Dr. Herbert Spencer (London) said he had not looked over his material to see in how many cases of unilateral ovariotomy for papilloma the other ovary had been affected, but his practice had always been in accordance with Professor Nagel's view, and he had never since the days of oöphorectomy for myoma removed an ovary which appeared to be normal, whatever the nature of the growth in the ovary removed. Papillomatous ovarian tumours were very common; in 200 cases he had found no less than one-fourth of the cystic tumours contained papilloma. Most of these cases were non-malignant, and he had seen a case with large masses of papilloma outside and inside the cyst which was quite well after seventeen years; on the other hand, he had seen a smaller tumour, to the naked eye and microscope exactly similar, in which the patient died in a few months with the pelvis full of growth. In these cases he did not think the removal of the other ovary would prevent recurrence, for he did not think, when both ovaries were carcinomatous, it was sufficient to remove the ovary; in these cases the uterus should be removed as well. This, however, was not the question raised by Professor Nagel.

Dr. Walter Tate (London) was of opinion that in the majority of cases of malignant papillomatous cyst of the ovary the disease affected both ovaries, and there was therefore no chance of saving one. These cases usually occurred in patients between 35 and 50 years of age, and clinically showed rapid formation of ascites. The cases in which intracystic papillomatous growth was found affecting one ovary were of a different character and presented all degrees of malignancy, and it was not possible to say whether they were malignant or not till they had been examined microscopically. In those cases he would agree with Professor Nagel in leaving the other ovary if it appeared to be healthy.

Dr. Blair Bell (Liverpool) said that the surgical procedures were in this, as in all cases in which neoplasms were dealt with, based on pathological considerations. The question of "malignancy" in papillomatous ovarian tumours had never been accurately defined. There were undoubtedly two classes of growth—the locally malignant or innocent disease and the adeno-carcinomatous. Sometimes these would be distinguished macroscopically during operation; often this was impossible. Sections, however, readily demonstrated the nature of the growth. The usual teaching that proliferation of the surface epithelium was evidence of real malignancy was erroneous, for this was a condition usually seen in "innocent" villous growths of the bladder. Absolute malignancy only existed when the stroma was invaded. A rapid section at the time of operation might decide the question in many cases, but a negative result was not to be relied upon, as adenocarcinoma might be confined to a small portion of the tumour. The ovary should always be left when the tumour on the other side appeared to be innocent; but if the growth was definitely malignant, or the second ovary showed the slightest departure from the normal (such as a small cyst) both ovaries and uterus should be removed. It would probably be wise to make this radical operation the routine practice in cases occurring in women over 40 years of age.

Dr. Louise McIlroy (Glasgow) said that, from examination of numerous specimens of carcinoma of the ovary, she had come to the conclusion that the disease in the majority of cases was bilateral, as in some specimens examined the opposite ovary, although appearing perfectly normal to the eye, showed early carcinomatous degeneration under the microscope. Professor Nagel had not drawn a sharp line between malignant and non-malignant papillomatous ovaries; but in one case in which the speaker had assisted at the operation ten years ago there was a large adeno-cystoma of one ovary removed; the other ovary, which did not appear malignant, was left *in situ*. Eight years later the patient returned with a mass in the pelvis and carcinomatous nodules in the peritoneum with ascitic fluid. She died six months later. The source of the disease was certainly the ovary which had been left. She also would advocate, in a doubtful case, not only the removal of the opposite ovary, but the uterus at the same time, for in two cases examined by her there was malignant infiltration of the broad ligament and uterine wall.

Dr. Felix Meyer (Melbourne) said that while Professor Nagel's collected experience was in favour of leaving the other ovary, an absolute rule could not be established. In this contention he found support in the remarks of Dr. Louise McIlroy. Macroscopic appearances were often misleading; he recalled two cases in point—one that of a young woman of 24, where on conservative principles he left the small apparently healthy ovary, with the result that within a few months of the operation the pelvis filled with an inoperable growth. In the other case he removed a large proliferating papillomatous cyst of the ovary in a woman of 54, leaving the other ovary, which appeared quite normal. From this ovary there developed an adeno-carcinoma, which recurred after removal, filling the abdomen. In his opinion, the uncertainty of macroscopical appearances should be as far as possible minimized by microscopical examination of sections of the second ovary made rapidly at the time of operating. He would like to share Professor Nagel's favourable view, but could not accept it as final. In the case the speaker had mentioned there was no doubt that the recurrence originated in the ovary left, and not in the stump of the one removed.

Professor Nagel, in reply to Dr. Munro Kerr, said he had certainly seen cases of far advanced carcinoma with infection not only of the other ovary but also of the uterus, and quite agreed that in such cases hysterectomy must be performed. In reply to Dr. McIlroy, he said that nearly 50 per cent. of all papillary ovarian tumours were carcino-

matous, but if only one ovary was affected at the time of operation, the danger of the other undergoing malignant changes also was not very great.

INVOLUTION OF THE UTERUS.

By C. Nepean Longridge, M.D., F.R.C.S.,
Cheltenham.

If one reads the accounts of involution in modern textbooks one fact seems to stand out from the description given—namely, that the uterus loses half its weight in a week. For the sake of exactness I may quote Whitridge Williams, who states that after delivery the uterus weighs 1,000 grams; at the end of the first week 500 grams, 375 grams at the end of the second week, and at the end of six to eight weeks when fully involuted it weighs 60 grams.

One can therefore distinguish two distinct stages in the process of involution—a rapid stage lasting about a week, during which the uterus loses 50 per cent. of its weight, and a slow stage, lasting about six or seven weeks, during which the uterus loses 45 per cent. of its weight. No explanation of the physiology of this process is to be found in textbooks. Involution is, of course, a physiological process, and I venture to state that as such it is perfectly unique, since no other healthy organ in the body can lose halt its weight in a week. A satisfactory explanation can be found in the application to the problem of the known facts concerning autolysis of tissues. The little knowledge of autolysis which is necessary may be summed up in a few words. If an extract with normal saline be made of a piece of fresh tissue a certain amount of nitrogenous extractives can be obtained in the filtrate from the extract. If, however, taking every precaution against bacterial action, one incubates the tissue with saline for some hours and then examines the filtrate the amount of extractives which can be obtained is very much increased. Therefore, some change has taken place in the tissue which is independent of bacterial action, and which results in the transformation of insoluble nitrogenous bodies into soluble nitrogenous extractives. This change is called autolysis. The rapidity of the change is determined by definitely ascertained factors. In an acid medium the change is much more rapid than in an alkaline one, and the presence of fresh blood or serum causes a marked diminution in the rapidity of the process. It is not certain whether the change is due to the action of tissue ferments which are set free from their zymogen by an acid, but such a theory provides a sufficient working hypothesis.

If we apply this knowledge to the involuting uterus the problem is at once simplified. After delivery the uterus is anaemic and has developed in its walls a certain amount of sarcolactic acid due to its muscular contractions—conditions which are ideal for rapid autolysis to take place. It seems, therefore, that the explanation of the first or rapid period of involution lies in the fact that the uterus is anaemic and of an acid reaction during the first five or six days after delivery. At the end of this period it is conceivable that the uterus has softened down to such an extent that the blood begins to circulate again through the uterine walls. When this happens the second or slow stage of involution begins, because the reaction of the tissue becomes alkaline. It is further conceivable that at the re-establishment of the uterine circulation the products of autolysis—that is, a considerable amount of nitrogenous extractives—should find their way into the blood stream and be excreted.

Such, then, is the explanation of involution when looked at in the light of autolysis, and there is a certain amount of evidence which appears to support it. First of all, as regards the anaemia of the uterus, I have been met with the objection that the uterus cannot be anaemic after delivery because lochia and clots come out of it. That objection can be met in several ways: First, inspection of the parts with a speculum will assure any one that the blood in the lochia is as much due to small lacerations of the vagina and cervix as it is to leakage from the uterus. Secondly, Nature has, as I have shown elsewhere, provided a double line of defence against bleeding, so that the uterus may remain "blood-tight" after delivery. Thirdly, I have myself had the not uncommon experience of attending cases of labour in which the patient had no lochia at all with the exception of a little mucous discharge; and this experience is shared by all veterinary surgeons, who find that it is a most uncommon thing for any cow, mare, bitch, or other female mammal to have any discharge after labour comparable to the lochia of the human female. Therefore there are grounds for believing in the possibility of a blood-tight uterus. Moreover, in one or two of the cases I have had in which there has been an absence of lochia I have noticed a slight red discharge at the end of the first week, which was probably due to the re-establishment of the uterine circulation.

Two or three years ago I was enabled with the help of a grant from the British Medical Association to carry out some such dealing with the excretion of nitrogenous material during the puerperium. To be brief, I may say that the work was carried out upon the puerperal women who, with the kind permission of the late Dr. Rivers Pollock, I placed upon a creatinin-free diet, containing 13·5 grains of nitrogen a day. The total nitrogen and creatinin excretion were determined in these patients daily, and the result of the research went to show that there was no rise in the excretion of endogenous creatinin, but that about the end of the first week of the puerperium there was a marked rise in the excretion of total nitrogen. This rise occurred in every case examined, and the time of its occurrence coincided with the period at which the uterine circulation becomes re-established and washes out the products of autolysis.

More work remains to be done before the seal of certainty can be set upon these views, but they appear to me to afford a reasonable explanation of the facts of involution, and from them a practical lesson in the management of labour may be deduced. It is to aim at a dry and blood-tight uterus at the end of labour. I think that we should hold the uterus for at least an hour after delivery, and then give ergot the charge of holding it for us; also that we should endeavour to finish a labour with a uterus as strong and as full of "buck" as possible, by not letting the uterus tire itself unduly; and lastly, that we should not allow the uterus to be pushed up into the abdomen by a full bladder. By these means the uterus is kept anaemic, and the first stage of involution is able to pursue its rapid course.

THE SURGICAL TREATMENT OF FIBROSIS OF THE UTERUS.

By A. Louise McIlroy, M.D., D.Sc.,
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The term "fibrosis" is applied to that condition of the uterus in which the whole organ is uniformly enlarged, hardened, and the walls under the microscope show increase in the stroma with or without muscular hypertrophy and thickening of the arterial coats. Clinically there is menorrhagia with dysmenorrhoea. I do not propose to enter into a discussion as to the use of the terms "chronic metritis" and "arterio-sclerosis of the uterus," a complete account of the literature and pathological condition being found in the papers by Gardner and Goodall,[1] Donald, Shaw, and Macdonald.[2] It is only necessary to state that the term "fibrosis" is applied to a uterus which has an increase in the fibrous stroma. Other areas in the same uterus may show hypertrophy of the muscular tissue and thickening of the outer coats of the vessels.

In taking up the treatment I have excluded those cases which were associated with marked pathological changes in the uterine appendages, and have confined my remarks to those cases where the uterus is the primary organ affected. For the treatment of uterine haemorrhage when not due to malignant or benign neoplasms the usual routine method is that of drugs, followed in some cases by the use of the curette. In cases of fibrosis this mode of treatment is of little use, the patient's symptoms being relieved for a time only, often to return with all the greater severity. In the early stages of the disease, when congestion is present and when the uterus tends to become retroverted, ventrisuspension may be of use; as far, however, as my experience goes, this operation is not of permanent benefit.

Fibrosis usually occurs in women during the child-bearing period of life, and for this reason the expectant method of treatment—that is, by waiting for the onset of

the menopause—is to be condemned, as the patient is a chronic invalid from repeated and prolonged haemorrhage.

The surgical treatment of fibrosis of the uterus resolves itself into two divisions—namely, removal of both sets of appendages in order to get rid of the haemorrhage, or the extirpation of the uterus along with or without one or both ovaries. In the first division it is true that in the majority of cases the haemorrhage is stopped, but the patient may still complain of pelvic pain, leucorrhoeal discharge, and general discomfort. By the operation of hysterectomy the pathological tissue is altogether removed, and if the patient be a young woman one or both ovaries may be left, thus obviating the discomfort of an artificial menopause.

Four cases of fibrosis of the uterus have come under my care, and in one of these I was enabled to verify my diagnosis by subsequent examination of the uterus by means of the microscope. In all four cases there was marked menorrhagia with dysmenorrhoea. Two were young unmarried women and two were multiparae.

In Case I the patient, aged 25 years, was curetted without any relief for the pain and haemorrhage. Eighteen months later I performed ventrisuspension of the uterus, that organ being enlarged and hard. There was no disease of the appendages. The patient reported herself as improved for several months, but complains of a return of the pain at her menstrual period.

In Case II the patient, aged 30 years, had suffered from dysmenorrhoea and menorrhagia for two years, the haemorrhage being alarming at times. I curetted the uterus and suspended it to the abdominal wall, and removed at the same time a small haemorrhagic cyst of the right ovary. The patient reported a return of the symptoms a few months after the operation. Owing to the age of the patient I was unwilling in either case to perform a sterilizing operation.

In Case III the patient, aged 37 years and multiparous, had an abdominal section performed by me for the suspension of a retroverted uterus. Both ovaries were found to be small and sclerosed, but were left *in situ*. The patient's symptoms still continuing, I removed both sets of appendages nine months later. There was a subsequent cessation of the haemorrhage, but some pelvic discomfort is still present.

In Case IV, a multipara aged 42 years, the patient had been curetted by a gynaecologist three and a half years previously. I advised a second curetting with the object of examining the endometrium microscopically. An interstitial endometritis was found, and a few days later the uterus was suspended, as there was some retroversion present. The uterine appendages appeared perfectly healthy. The patient was kept under observation, but the menorrhagia still persisted, and I advised hysterectomy with removal of both appendages, which I performed over a year ago, and the patient has reported herself to be perfectly well.

In two of the cases the operation of curetting and subsequent ventrisuspension gave temporary relief only. In one case oöphorectomy gave relief from the haemorrhage, but not from the pelvic pain. If combined hysterectomy had been performed in this case the pain would have disappeared. In the fourth case a complete cure was obtained by hysterectomy. The point of importance in the treatment of cases of fibrosis is the early recognition of the condition, and although it seems a somewhat drastic procedure the operation of hysterectomy is the only one which will give a satisfactory termination to the symptoms.

REFERENCES.
[1] BRITISH MEDICAL JOURNAL, November 3rd, 1906. [2] *Journ. of Obstet. and Gyn. of Brit. Emp.*, vol. xi, 1907.

DISCUSSION.

Dr. WALTER TATE (London) said he had seen many cases of fibrosis of the uterus, and in his opinion, when medical treatment and curetting the uterus had failed, removal of the uterus was the only operation to recommend. He would not expect any improvement from the operation of ventrifixation when the patient was suffering from menorrhagia and dysmenorrhoea. The majority of cases who came up for treatment were about 40 years of age; but it was to be remembered that the menopause was often delayed till well over 50 in many of these cases, so that they could not be left to wait for natural cures. In the case of young girls the matter became a very serious one, particularly when they had to earn their living, and the severe menorrhagia and dysmenorrhoea made this quite impossible.

Dr. COMYNS BERKELEY (London) had performed hysterectomy for several cases of fibrosis of the uterus after all other methods of treatment had failed. He did not understand how ventrifixation or suspension could relieve these cases, and he would not expect it to do so, neither did he think removal of the ovaries justifiable. An alternative treatment to hysterectomy which should be seriously considered was the removal of a wedge-shaped piece of the uterus, after the method of Kelly, so that its size and cavity were materially reduced. He had knowledge of three cases in which this treatment had been very successful.

Dr. CURTIS WEBB (London) said he ventured to speak on the subject of this paper on account of one case that he had treated by intrauterine electrical applications of the constant current. The case had been sent to him by Dr. John Phillips, who had made a diagnosis of fibrosis uteri.

The patient, a lady of 47, had had four children and seven miscarriages, the last at three months six years ago. For three years there had been menorrhagia, the periods lasting ten days. Afterwards metrorrhagia came on; for the past year the haemorrhage had been almost continuous, so that the patient, who lived in the country, had for the eight months previous to coming to London for the consultation been confined to bed and sofa in one room. After examining the case, Dr. Phillips advised hysterectomy, but, as the lady was very anxious to avoid operation, and as she was very anaemic, she was sent to the speaker for electrical treatment. A zinc intrauterine electrode of the speaker's own design, and of the largest size that could be painlessly inserted into the uterus, was used. This sound was amalgamated with mercury, and a current of 100 milliampères passed for eight minutes to each successive segment of the cavity. Treatment was commenced on January 21st, 1908, and repeated three times a week for five weeks. Haemorrhage ceased on February 3rd, and the patient returned home on February 26th. From that time onwards the periods were regular, lasted four days without any excessive loss till February of the following year, when they ceased. The patient made an uninterrupted return to health and vigour.

It was known that during the passage of a constant current there was a determination of the acid radicles of the constituent salts of the body fluids towards the positive pole, and it was the cauterizing and styptic effect of this acid that brought about the haemostatic action that always occurred at this pole; but no doubt the ionization of the zinc and mercury ions had a strong antiseptic action on any irritating bacterial infection that might have been present. It must be remembered that, if it was desired to resolve fibrous tissue by ionic methods, the negative pole must be used, and one would like to be able to employ this procedure, but one was barred by the fact that this pole encouraged haemorrhage. The speaker entered a strong plea for the judicious use of electrotherapeutics in suitable gynaecological ailments, markedly in certain forms of haemorrhage. He did not know whether ionization was a surgical procedure because instruments were used, or medical because chemical substances were introduced. Perhaps it might be called a "borderland" treatment, and he feared that, in the opinion of many gynaecologists, those who considered that electro-therapeutics could play any useful rôle in the treatment of gynaecological affections were themselves "borderland cases"—in the alienist's use of the term.

Dr. HERBERT SPENCER (London) agreed with Dr. Tate that no good was to be expected from ventrisuspension or ventrifixation in these cases. The cases were not rare, and were very intractable. He had been driven to do hysterectomy in these cases, but was not proud of it—indeed, he thought it was an operation of which gynaecologists ought to be ashamed. He had only done it with great reluctance, and thought it should not be done in any young woman, and that in women approaching the menopause it would be wise to wait as long as possible. He had seen remarkable results in a few cases from steaming the uterus; in one case—the only case of haemophilia he had seen—the patient remained quite well after many years. The operation, of course, had the drawback of sometimes causing stenosis. He also thought that Dührssen's operation of excising portions of the mucosa and Kelly's operation of making a utriculus were preferable to the removal of the uterus.

Dr. BLAIR BELL (Liverpool) expressed surprise that none of the previous speakers had called attention to the fact that not only was there no evidence of "fibrosis uteri," in the true sense of the term, in 3 out of the 4 cases

quoted by Miss McIlroy, but, further, that in point of fact 2 of the cases could not have been of this nature. The term had been seized upon as an explanation of many cases of menorrhagia and metrostaxis, without the slightest justification. For this reason also the question of treatment had not received proper consideration in the discussion. If care were taken to consider the disease from a proper pathological standpoint the best methods of treatment could not remain in doubt, and the question of removing the uterus from young married women should never arise. The condition termed "fibrosis uteri" only occurred, in the vast majority of cases, in women who had been pregnant. It was due to three contributory factors: (1) Imperfect involution, probably the result of slight (2) infection subsequent to parturition; (3) uterine fibrosis in excess of the normal age changes. The condition was therefore usually found in women over 40 who had borne children. The uterus was usually normal in position or anteflexed; it was extremely rare to find the organ retroflexed. The treatment consisted, first of all, of curettement and the administration of calcium lactate (gr. xxx every night for a week, and then on alternate nights for a month or two). Should this treatment be not entirely successful, ionization of the uterus should be tried; this often gave excellent results. In extreme cases hysterectomy should be performed to save the patient from the excessive loss of blood. This operation was perfectly justifiable, since the woman had usually almost reached the climacteric age and had had children. In the exceptional cases in young women in which fibrosis was present it was due to metritis, but this itself was excessively rare in nulliparae, for gonorrhoea (the common infection in this class of patient) rarely affected the muscle walls of the uterus except after parturition. In "congenital" flexions and in infantile uteri there was, of course, more fibrous tissue than normal, but this was not the same condition as acquired "fibrosis uteri," nor was there menorrhagia with these conditions. The uterine haemorrhages seen in young women were frequently not local in origin, but arose as the result of derangements of the ductless glands, and should be treated on appropriate lines; this subject was beyond the range of the present discussion.

Dr. PURSLOW (Birmingham) said he had published two of the earliest recorded cases of fibrosis of the uterus eleven years ago in the BRITISH MEDICAL JOURNAL, June 10th, 1899, under the heading of Two Cases of Intractable Uterine Haemorrhage in which Vaginal Hysterectomy had been Performed. He had been compelled to perform hysterectomy on several cases since then, but these had all been in parous women, and he had not so far found it necessary to perform this operation on a nullipara. As regarded drugs for uterine haemorrhage, he had sometimes seen good results from styptol (cotarnine phthalate) when ergot and other drugs had failed.

Professor J. A. KYNOCH (Dundee) said that in cases of fibrosis of the uterus in which all medical means had failed to arrest the haemorrhage and cure only resulted after hysterectomy he considered the radical treatment justifiable, especially in hospital patients. By removing the uterus and leaving the ovaries the results were most excellent. Radical treatment was, of course, only justifiable as a last resort.

Dr. HASTINGS TWEEDY (Dublin) was glad to hear Dr. Herbert Spencer approve of vaporization of the uterus. He had seen most remarkable results from the application of superheated steam in menorrhagia and metrorrhagia. The treatment was painless and apparently perfectly safe when employed with due and necessary precautions. A uterus should never be removed for fibrosis until steaming had failed to effect a cure; when it was tried there would be no occasion to remove the uterus afterwards.

Professor GOTTSCHALK (Berlin) recommended in cases of severe bleeding sterilized injections of gelatine.

Mr. S. G. KIRKBY-GOMES (London) had under his care at the present moment a case which exhibited the symptoms described by Dr. McIlroy, though the uterus was anteverted, not retroverted. It was one of three cases of fibrosis of the uterus which had come within his personal observation. The present case had severe haemorrhage and had been in the hands of a medical practitioner for several months. All available remedies supposed to be suitable, including styptol, had been applied, but without result. She was now in the speaker's hands for surgical treatment. He had thought of trying Kelly's method, but having heard what Drs. Herbert Spencer and Tweedy had to say about atmocausis in these cases, would try this before going any further. He regarded both ventrifixation and oöphorectomy as quite unsuitable in connexion with cases of this order, and hysterectomy as an unsurgical procedure in fibrosis, if that condition was really a pathological entity, in a woman under the age of 55.

Dr. MARY SCHARLIEB (President) could not understand why ventrifixation should be expected to cure a case of uterine haemorrhage if the cause were degeneration of its vessels. She agreed that drugs, curetting, and ionization should first be tried, but if a radical operation were necessary it was surely better to remove the uterus, or to form a utriculus by Kelly's method. Oöphorectomy might fail to arrest the haemorrhage, and deprive the woman of the internal secretion of the ovaries, a product which appeared to be of value. The removal of the uterus certainly arrested the haemorrhage, and grievous as it was to remove that organ, its loss had less evil influence on the woman's health than was caused by removal of the ovaries.

Dr. A. LOUISE MCILROY, in reply, said she had limited the discussion to the surgical treatment, as it would have been interminable had drugs and local applications been brought forward. In reply to Dr. Herbert Spencer, in the case of young women where ventrisuspension had been done there was slight retroversion of the uterus. The application of medication to the endometrium might be associated with an increase in the severity of the symptoms. That hysterectomy should be the last resource only was the general opinion of the meeting, but much valuable time might be lost by postponing this operation for a length of time. Dr. Blair Bell had brought up an interesting question—namely, the relationship of the thyroid gland. The speaker herself had under treatment at present a case of uterine haemorrhage which she was treating by thyroid extract, and at present it seemed to act as relief to the symptom.

DEMONSTRATIONS.

I.—EROSION OF THE CERVIX.

PROFESSOR SIGISMUND GOTTSCHALK (Berlin) gave a lantern demonstration of photomicrographs illustrating various stages of erosion of the cervix, and formulated the following conclusions. Erosion of the vaginal portion of the uterus is inflammatory, and due to irritation; the glands in the erosion are true ectopic cervical glands. The normal stratified squamous epithelium perishes owing to (1) subepithelial extravasation of blood; (2) copious round cell subepithelial infiltration, or (3) rupture under pressure of retention cysts arising from such cervical glands (ovula Nabothi). The nude surface of the vaginal portion becomes covered, in a secondary manner, with cylindrical epithelial cells, with vibratile cilia arising from the ectopic cervical cells, which are opened on the nude surface. In some instances cylindrical epithelium may arise from the surface of the cervical mucosa also, but the squamous epithelium of the portio is never transformed into cylindrical secreting epithelium, and is never the origin of glands deep in the connective tissue (Ruge, Veit). In the third stage, newly formed stratified squamous epithelium is pushing off the cylindrical epithelium from the surface of the portio, and sending out strong papillae deep into the connective tissue, and crushing the ectopic glands. There is a contest between the two forms of epithelium in which the newly formed squamous epithelium usually gets the victory. But this contest may not always be without danger to the patient, for malignant degeneration may occasionally take place. It is therefore better in such severe cases of glandular and papillary erosion of the portio to make a wedge-shaped excision of the diseased tissue.

II.—EXTRAPERITONEAL CAESAREAN SECTION AND HEBOSTEOTOMY.

PROFESSOR DÖDERLEIN, of Munich, exhibited a series of pictures, illustrating his extraperitoneal method of performing Caesarean section and subcutaneous hebosteotomy, which he explained was the correct term to apply to what was sometimes called pubiotomy. Descriptions of these operations would, he said, be found illustrated and set forth in the third edition of the book on operative gynaecology by Krönig and himself.

In the discussion which followed, Dr. HASTINGS TWEEDY said obstetricians had to thank Professor Döderlein for the very great help he had given them by introducing the operation of pubiotomy. It was a most satisfactory operation, but in the speaker's opinion the true subcutaneous method of performing it had many marked advantages over that now demonstrated. It was so easy to perform that any well-trained practitioner should be prepared to undertake it. There was little haemorrhage, and the convalescence was easy and painless. Hysterotomy, too, was a splendid procedure, and could be undertaken in cases where classical Caesarean section would be most dangerous. The speaker had successfully performed three such operations, two symphysiotomies, and five pubiotomies, and so far as convalescence and subsequent results went, there seemed to be nothing to choose between any of them. Dr. MUNRO KERR agreed that pubiotomy was a most valuable operation, but did not regard it as one which a practitioner could employ with only the ordinary appliances to be found at the patient's home. He had employed Professor Döderlein's method, and believed it to be the best method though it could not accurately be called subcutaneous. The method of performing Caesarean section described should certainly be tested by all obstetric surgeons, but he was doubtful if it would prove to be such a great advantage. Ordinary Caesarean section gave most excellent results in all except infected cases. It did not seem likely that this method of hysterotomy would be attended by less infection. It opened up the cellular tissues and their infection could not possibly be prevented. It was, moreover, a complicated procedure. Dr. HERBERT SPENCER agreed with Dr. Munro Kerr that probably there was no great advantage in the extraperitoneal Caesarean section; but it had also, he thought, the disadvantage that it took longer to deliver the child. He (the speaker) had often delivered the child by the classical operation in one minute or a few seconds more. He thought it important that the child should be delivered quickly, so as to avoid asphyxia. Professor DÖDERLEIN, in reply, said that in his method of performing Caesarean section the time occupied, from the first incision to the delivery of the child, was about five minutes.

SECTION OF PSYCHOLOGICAL MEDICINE AND NEUROLOGY.

THEO. BULKELEY HYSLOP, M.D., C.M., F.R.S.E., Senior Physician, Bethlem Royal Hospital; Lecturer on Mental Diseases, St. Mary's Hospital, President.

PRESIDENT'S INTRODUCTORY REMARKS.

As President of this Section, allow me to bid you a hearty welcome to this our annual gathering.

In view of the fact that anything in the nature of a presidential address is discouraged by the Executive Council of the Association, it appears to me that I shall best serve the wishes of all concerned if, instead of addressing you on any special topic, I confine myself to a few remarks on the general philosophy of the subject-matter with which our combined Sections of Psychology and Neurology have to deal.

Firmly convinced, as I am, that no great or effective progress can be made in any one science otherwise than by a method and logic common to it with all others, I propose testing the present state of the relationship of psychology to neurology by the light of those principles of universal philosophy which have guided the sister sciences of Nature to their present state of maturity.

Within comparatively recent years the experimental and statistical study of mental phenomena, aided by measurement and mathematics, has enabled psychology to rank as one of the exact sciences—a position which the philosopher Kant expressed himself in despair of its ever reaching. The numerous physiological and psychophysical laboratories which have sprung into existence, and the methods whereby an empirical psychology without metaphysic—or a psychology without a soul—have served, as Professor James says, "to pull the pall over the psychic half" of the phenomena, have enabled psychophysics and even psychology to assume a dignity and importance all their own, and which would appear to place them outside the realms of conjecture, metaphysics, and philosophy.

The total exclusion of metaphysics from the so-called exact sciences is in reality however, an impossibility. The physicist accepts mere hypotheses, such as those of the existence of luminiferous ether, conservation and correlation of energy, gravitation, etc., as hypothetical entities on which to base his science, denying at the same time any hypothetical entity, such as the soul or spirit, on which the psychologist would, or ought, to base his science.

It is not my object, however, to plead either on behalf of psychology or of physiology. A commonsense and uncritical dualism would appear to be the legitimate position for the worker in science, and it is only when this standpoint is exceeded, and, in an unthinking way, all epiphenomena and their metaphysical bearings are condemned, that a halt should be called, and that they should be told to refrain from giving judgement on matters outside their own legitimate province.

The "principle of concomitance" or of "psycho-physical parallelism" is now regarded as being at the very base of modern psychology, and to some writers this acknowledged principle of complete psycho-physical parallelism is a crushing refutation of all metaphysics whatever. Let us look, however, at the question more closely, and we shall see that, no matter how perfect the concomitance or parallelism may be, we are still very far short of any solution of the main problems of life and mind.

With regard to the evolution of mind, or the dawn of consciousness, from the world of inorganic matter, with its stores of indestructible energy, two hypotheses present themselves:

1. Mind existed and exists throughout the extent of space (if there are limits) and through all time from the very beginning (if there ever was a beginning), as a hypothetical entity differing from, and yet for its manifestations depending upon, other hypothetical entities such as energy, gravitation, electricity, and even life itself. That is, mind has had an existence parallel to that of energy and of the various other hypothetical entities granted by the so-called exact sciences. This would form the basis for the hypothesis of an exact and complete parallelism, and it would have no limits in time or space. Mind would exist as a hypothetical entity, being neither translated from nor into any other hypothetical entity whatsoever.

2. The other hypothesis would be that mind is evolved from energy as a transformation, and therefore comes within its law of indestructibility, which would necessarily involve the supposition that just as it is evolved from, so it must return to, the source from which it came. If the latter theory be true, it is but fair to assume that if mind is derived solely from physical hypothetical entities it must have existed as a potentiality concomitant to and dependent upon all space and time, and, moreover, it could never die, inasmuch as it would of necessity be merely translated back to the source from whence it came.

Energy, with its numberless transformations, knows no limits in space or time. Of the complex host of undulatory stimuli which are derived from myriads of sources to stimulate the respective organs of special senses and to be ultimately translated into the physical concomitant of a sensation in consciousness none are lost. Our physical organism is dependent on the stimuli of light and heat for its very existence. The energies it abstracts it also gives back, and the sum total of energy is always a fixed quantity. Mind, as a hypothetical entity, can neither abstract from nor add to the sum total of available energy.

For polemic purposes the two theories are about on a par. Mind is either coeval with the world or it is evolved in a manner which would necessitate either the supposition that it is created out of nothing, or, if out of something, the favourite dogma of the exact sciences as to the indestructibility of energy must be erroneous.

The physiologist climbs to the top of his own special ladder, and, when he has noted with care the characteristics of each particular rung, he is satisfied, and views all else in nature from the summit he has attained. Similarly, the psychologist climbs his ladder and, by noting and measuring each step of his progress, ultimately crows loudly from the topmost rung that he alone stands above all others. The dualist or psycho-physical concomitance advocate, on the other hand, binds the two ladders together, step by step, and rung by rung, and, when mounted at the summit, he is satisfied that parallelism is the one and only solution of the world's enigma of consciousness.

The enthusiast in psycho-physics—owing to the apparent completeness of the concomitance or parallelism, and even dependence—is apt to wander from legitimate conjecture into realms which are fanciful and even absurd. The parallelism can be traced so far and no further. The record written in the rocks of our own earth permits us to trace the history of the spread of life where inorganic and organic matter have been intimately combined, as if the inorganic were the structure or instrument for the manifestation of the phenomenon of the organic. But how are we to carry out the theory of parallelism in its entirety? An adequate theory would have to conceive the universe as being but a sea of innumerable waves of motion having various interpretations, as energy, gravity, electricity, co-extensive in space and in time with life and mind.

The universal laws of physics do not make exceptions for the benefit of life and mind, and it is just as inconceivable that any combinations or transformations of energies can produce life as that inorganic matter can produce energy, gravity, electricity, etc. In the abstract they are all capable of being spoken of in different terms, but in the sum they are interwoven so inextricably that they cannot be conceived as existing apart. Life and mind ought therefore to be as abstract principles, co-extensive in time and space with those other principles on which the physical sciences are based.

To regard mind as being evolved from energy, and as something different from and incapable of being spoken of in terms of energy, would necessarily involve the confession that the law of the conservation of energy is untrue, inasmuch as energy would be thereby surrendering itself in part to the construction of something totally different. Refusal to acknowledge this objection would necessitate the granting that if mind comes from energy in some subtle and inconceivable manner as a mere translation or transformation of energy, then it must assuredly return to the source from whence it came, and this would necessarily involve the conception that everywhere throughout space and time we have the potentialities of mind, in which case the one would be as indestructible as the other.

When we attempt to explain the so-called epiphenomena of mind and endeavour to find some physical basis for the alleged occurrences of thought transference and telepathy we can but confess to failure, or, at most, to some faint glimmer of insight as to possibilities. Physics have either helped or deluded us concerning ether, that bewildering hypothetical entity filling all space and permeating the inmost recesses of all matter, solid, liquid, and gaseous, with its function of propagating the transverse vibrations which constitute light, its duty of transmitting the pull of gravity from particle to particle and from world to world, and its myriads of phases and electrons as electricity.

Certain it is that modern research shows us that electric disturbances like those made use of in wireless telegraphy can be propagated, like light, at great velocities through vast distances, even though there be no tangible connexion between the source of the disturbance and the distant receiver. Similarly, biological science—and possibly psychical research also—has made us aware that both in the animal kingdom, and possibly also in man, communications can be made at great distances and apparently without the aid of any medium as yet hypothetically conceived by us.

We await the coming of another Newton who shall formulate for psycho-physics laws comparable to that of universal gravitation, and draw a comparison between the mechanism of the infinitesimally minute atoms and the infinitely great cosmos. In time it may yet be realized that the problems of life and mind are as dependent upon all-pervading principles as are the problems of gravitation, energy, and magnetism.

In psychological science we await the coming of a second Darwin who shall enunciate a theory as to the origin of *specific qualiae* (or the perception of qualitative differences between sensations). Up to the present all efforts to solve this problem have been futile, and philosophy seems to indicate that the main problem of the evolution of mind is as yet unassailed. The recognition that the nervous system is not a generator, but merely an instrument for the transmission of force (the so-called *vis nervosa*), is one step in the right direction towards a better conception of the possible relationships between mind and matter and the abstract principles pertaining to each. When life gives place to decay and its living combination of "organic molecules" again becomes transformed or translated to the so-called inorganic world, mere disintegration of structure does not destroy its energy, its magnetism or any of the ultimate chemico-physical factors upon which it depends for its being. It remains open, therefore, to ask whether life or mind can logically be assumed to be capable of being added to or subtracted from those other principles known to physics as quantitatively eternally fixed.

Philosophy, science, and even religion, therefore, must necessarily come in due course to terms of agreement. Philosophical psychology demands that physical science shall either agree to its claims for the existence of a soul or give up its greatest dogma—the indestructibility of energy. When philosophy and science shall meet on common ground, much that is now vague and uncertain must in time, by becoming definite items of knowledge, not only tend to abolish agnosticism, but even give rise to positive articles of faith.

DISCUSSION ON INSANITY AND MARRIAGE.

OPENING PAPER.

By G. H. Savage, M.D.Lond., F.R.C.P.,
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The subject of marriage and insanity is one which is at present beyond practical legislation in this country, but is of immense importance for the social stability and for individual welfare. Though we cannot legislate we have to advise, and our advice is sometimes taken. The lay public as a whole, and the majority of medical men, would at once reply to the question as to the insane ever being allowed to marry in the negative, but I shall contend that a dogmatic statement of this kind is unscientific and unpractical. Such a declaration is taking it for granted that all insanity is alike and is to be treated as of equal value. I shall begin, then, by saying that in my belief certain persons who have suffered from a degree of mental disorder which may be classed as insanity may yet recover and marry with no real increase of risk to their partner or to their children. If I say this, you will next expect me to give examples of the forms of insanity which I should consider as not being of vital importance.

First, then, I have seen a good many adolescent cases with melancholic symptoms, many with marked hypochondriacal ideas who have recovered, and have after the interval of some years married and have borne the burden and heat of the world with sanity and well-being.

Again, I have met with a good number of adolescent cases with maniacal excitement who have recovered completely and have had no recurrence of mental disorder, and who have married and have remained healthy and sane.

Rather apart from such cases, I have met with cases of women who, having had attacks of insanity before the menopause, have married when past child-bearing and have remained well; but few would recommend such unions.

I have seen a few cases in which there was acute delusional insanity, probably toxic in origin, in which recovery has taken place and in which satisfactory married life has followed, but I admit at once that I should hesitate before advising marriage in such cases. I think in all these cases the question of family neurosis must be considered, and would influence one's advice. Again, cases in which after recovery the patient has been free from dread of recurrence are more favourable than those whose dread may act as autosuggestion. Doubtless many of my audience can give examples of other forms of cases of insanity in which marriage might be justified.

And now as to the cases in which marriage should be hindered or prevented. In no case should it be allowed where there is a history of periodical recurrences, and it is certain that there is very grave risk in those cases of adolescents who at puberty and with adolescence have periods of depression and buoyancy. I have seen a good many such cases in which there has been marriage in haste with a leisure of repentance. I think suppression of the facts as to such attacks should really be a ground for declaration of nullity.

It is so firmly established in the public mind that direct heredity is the chief cause of insanity that the question as to marriage of persons with insane relations is constantly brought before one. It is impossible to prevent marriage of all related to the insane; if you did you would not benefit the race, and I have seen many instances of breeding out of neurotic types.

Neurotic heredity certainly must influence your advice when you have to decide when the patient has symptoms of some form of insanity, such as delusional insanity, which is generally met with in members of insane families.

I would never allow marriage in any cases where there are fully-organized delusions or hallucinations. I have met with cases in which a man has slowly isolated himself as the result of concealed ideas of persecution, and in one case I have known such a one marry with the one idea that he thus was getting a protector between him and his tormentors. In this case a nullity was decreed. I learnt some years ago of the danger of acting on what appeared to be justifiable grounds in such a case.

A middle-aged man of independent position had developed general alopecia, and had become terribly sensitive to the notice of the persons whom he met. Doubtless they looked at him, but he misinterpreted their looks and gradually shunned society. He would only go out after dark, and then alone. He had hallucinations of hearing, and was profoundly suspicious. He contracted influenza, which was followed by pneumonia, and during his very severe illness he was nursed most carefully by a woman of middle age. He was very grateful, and was persuaded that there was at least one human being who did not detest him. He offered her marriage, and after seeing her I decided that, being past child-bearing and understanding the possibilities of a future redevelopment of the disorder, she might marry him. It was arranged that in any case a fixed income should be settled on her. All went well for a time, but again the delusions arose, and he nearly murdered his wife, and had once more to be sent to an asylum. Next, I cannot think that marriage should ever be permitted in cases in which there has been epilepsy with any mental symptoms. I am in doubt whether any epileptic should be allowed to marry, but I have in my mind at present several instances of women subject to *petit mal* who have lived and had healthy children though epileptic. Yet the risk is very great. There can be no doubt that with any form of mental disorder epilepsy should be a bar to marriage.

It is, I admit, difficult, and apparently hard, to say to a girl of 20, subject hitherto only to nocturnal attacks, that she ought not to marry, but my experience makes me say this strongly.

I feel equally strongly about marriage in cases of moral perversions; I have seen so many wrecked lives of women who have married drunkards with the notion of reforming them. Marriage, like other events in life, may check a few in their downward course, but I should be strongly opposed to advise the risk to any one whom one loved. Again, in cases of sexual perversion and in cases of impotence the advice to marry is certain to be dangerous, or worse.

Nearly allied to these cases one has to consider those of general paralysis of the insane, and I fancy many of us have had difficulties in these cases. Men with early acute symptoms may apparently recover before you are really certain that the disease is general paralysis of the insane, and then after a short period with the feeling of buoyancy and with general amorousness the patient wishes to marry, and I have to admit that I have been obliged in some cases to say, "Well, I think it is a mistake, but I cannot be certain as yet that the disease is general paralysis of the insane." I have known disaster follow rapidly after marriage in such cases. In fact, I always feel grave risk in not preventing marriage in any case in which there has been clear evidence of constitutional nerve trouble depending on syphilis. I have seen miserable results following marriage of tabetic subjects, as well as in those who have had some local cranial nerve affections.

Marriage should never be recommended as a means of cure. In so-called hysterical cases the prospect even of relief is small and the risk of permanent alienation is great. I have seen several cases in which after such marriages there has been separation, and others in which application for divorce has been made. I would speak equally strongly against marriage as relief for so-called neurasthenia or hypochondriasis, and I have already said that for sexual disorder it is dangerous.

Allied to the above subject is that of insanity and divorce, but as this is likely to be brought before the Commission at present sitting I shall not speak on the matter.

I leave, then, the points for discussion to be:

Should any one who has been certified as of unsound mind be allowed to marry? If so, which cases or which forms of insanity may be considered as not barring healthy unions and healthy offspring?

Should not the recurrence of any form of insanity be a bar?

Should evidence of syphilitic affection of the central nervous system bar marriage?

Should marriage ever be recommended in cases of mental disorder?

Should insanity be a plea for divorce?

DISCUSSION.

Dr. Shuttleworth (London) said that his experience of the subject was derived from the care of the unfortunate products of ill-assorted marriages rather than that of the contracting parties. The well-known reticence (sometimes misrepresentation) of parents of mentally defective children as to the existence of neuroses and psychoses in their family histories tended to obscure exact statistics, but undoubted evidence existed as to the large part neuropathic heredity played in this matter. His friend, Dr. Fletcher Beach, and he had many years ago reviewed the history of 2,380 of their cases at Darenth and the Royal Albert Asylums respectively, and had arrived at the conclusion that at least 21.38 per cent. came of stocks in which there was a definite history of insanity or imbecility, and 20 per cent. more in which there was a history of epileptic or grave neurotic inheritance, making 41.38 per cent. in which there was evidence of serious nervous abnormality. Dr. Tredgold, who devoted much time as research scholar in tracing out to three generations the family history of imbeciles in the London County Council and other asylums, found there was a larger percentage of definite neuropathic inheritance—namely, 64.5 per cent. That neuropathic inheritance was a far more potent cause than the personal mental conditions of the parent was shown by another study of his as to children born in asylums of insane mothers. In 38 cases of this kind, of 14 born of mothers whose insanity was of an accidental or transient character, 10 were passing through childhood without evidencing physical or mental impairment and there was no trace of morbid inheritance, and 4 died. In the other 24, in which there was a neuropathic family history only, 3 were alive and well; while 21 had succumbed, with one exception, a few months after birth. The mating of neuropathics should undoubtedly be discouraged by public opinion if not by legislative enactment. The mutual attractiveness of neurotics for each other was long ago remarked by Dr. Clouston, and attachments between such should, if possible, be nipped in the bud. Consanguineous marriages, where there was a neuropathic tendency (what

family nowadays, as a learned judge lately remarked, had not at least a second cousin insane), should be specially denounced. In-and-in breeding had had a baneful effect in certain communities—for example, the Society of Friends. The speaker also adduced instances from his practice in which a sort of craze for cousin marriages extending over more than a single generation had been productive of the most disastrous results to the physical and mental condition of the progeny. He supposed the time was not yet ripe here for laws regulating marriage and prohibiting that of the unfit, such as even now were in force in some of the American States; but it was incumbent on the medical profession to do their utmost to educate public opinion on the subject.

Dr. Robert Jones (Claybury), whilst agreeing that in the main the marriage of people who had been insane was undesirable, and that he would consider delusional insanity (paranoia) and all kinds of epilepsy as an absolute bar, thought that the tendency of Nature to an average type should not be overlooked—that is, that the progeny even of those with a neuropathic heredity might be normal. Mendelism, which ought to be a help, was rather unfair in that it took too exclusive cognizance of the parents and neglected the fact that there was no such thing as gametic purity. From a study of his own cases he had arrived at the opinion that heredity was not so important a factor as was generally esteemed, and that the really important determining factors of insanity were the influences of environment, alcohol, and tubercle.

Dr. Risien Russell (London) said that he was, of course, more especially interested in the questions raised in regard to epilepsy, syphilis of the nervous system, and impotence. Although it was his custom to discourage the marriage of epileptics, he thought that some distinction ought to be made between confirmed epileptics or those with a bad family history and what he might possibly be allowed to speak of as curable cases—on much the same lines as Dr. Savage had advocated in cases of insanity. He thought that permission to marry could not reasonably be withheld from a patient who had suffered from only a few epileptic attacks and in whom there had been no recurrence for several years after all treatment had been discontinued. It seemed to him that the same must be said of syphilis of the nervous system, as there were cases of the kind that were capable of cure under proper treatment. He did not, of course, refer to such late sequelae of syphilis as general paralysis of the insane or tabes dorsalis. As to impotence, he thought that a clear distinction ought to be drawn between congenital and acquired cases, and in the latter group a further distinction should be made, both in regard to the age of the patient and the cause that had been responsible for the condition. The congenitally impotent should, of course, never be allowed to marry, and the same rule should apply to acquired cases in which the condition still obtained. But when a cure had been effected in an acquired case, the person ought to be allowed to marry.

Dr. Fletcher Beach (London) was of the opinion that syphilis in the parents was not a cause of mental defect in the children. Some years ago he inquired into the subject, and was surprised to find that only about 2 per cent. of mentally defective children had parents in whom syphilis had existed. In this opinion he was fortified by other authorities, such as Dr. Shuttleworth and the late Dr. Langdon Down.

Dr. Greenlees (London) said that in South Africa representations were made to the Government with the object of introducing legislation to prevent consanguineous marriages in a race with marked neurotic tendencies, or else to provide adequate accommodation for the progeny of such marriages. The Government declined to legislate on the matter, but provided excellent accommodation for imbeciles.

Dr. David Blair (Lancaster) maintained that insufficient use was made of the force of public opinion in attempting to check the marriage of the unfit, and that every one before marriage should be obliged to take medical advice as to their fitness.

Dr. Stanley Barnes (Birmingham) expressed the opinion that a large group of insane and epileptic individuals need not be debarred marriage. He referred to those cases in which insanity or epilepsy had developed as the result of some great mental or physical shock. He instanced cases of temporary insanity following fractured skull or cerebral laceration, and also those who had been subjected to the excessive physical and psychic stresses of the battlefield.

Dr. A. R. Douglas (Royal Albert Asylum) agreed with Dr. Blair that many marriages of unfit people were due to ignorance, but suggested the establishment of local authorities in place of individual medical men for the pronouncement of a definite affirmative or negative opinion on the question of the marriage of persons with marked stigmata of degeneration.

The President, in closing the discussion, said that the time for legislation was not yet ripe. By the efforts of the medical profession the public had been enlightened as to the evil effects of alcohol, and legislation had followed as a matter of course. Similarly with regard to phthisis and many other important factors in the etiology of disease, all of which were now being adequately dealt with, but only after public opinion had been fully educated and public sympathies enlisted. He believed that the Church and medicine would fall into line as to the prohibition of marriage between certain degenerates. The Church was glad to thwart all departures from biological laws which were natural laws, and, therefore, for them, "laws of God." In fact, the Church would welcome any legislation which would relieve it of its great responsibilities in this connexion. Unfortunately, they had to recognize the fact that very few of those who consulted physicians as to whether they should be allowed to marry ever took the advice given them.

PARANOIDAL SYMPTOMS AT THE FEMALE CLIMACTERIC.

By Leonard D. Baugh, M.B., Ch.B.Edin.,
Senior Assistant Medical Officer, Glasgow District Mental Hospital.

Systematizing of delusions is a symptom often present in insanity; it, like other signs of mental abnormality, is frequently intensified at the climacteric. If systematizing starts during this epoch the relationship is usually incidental; on the other hand, a few cases seem to be definitely associated with menstrual cessation; to the consideration of these attention will briefly be paid.

In the cases regarded as definitely associated with the climacteric I hope to show the marked blending of systematized delusions and depression. Illustrations of it came under notice when examples of depression were being investigated, and in a paper read to this Section of the Association at Sheffield in 1908 they were briefly referred to as paranoidal.

Paranoidal symptoms appear to be only met with in some of those in whom mental illness occurs for the first time during the climacteric period. They are certainly not common; nevertheless, they are very marked, and have not received the mention in literature they merit. The blending of symptoms termed "paranoidal" must not be confused with systematization found in:

(*a*) Some melancholics where ideas of persecution are systematized to even a considerable extent.
(*b*) What some have called degenerate paranoia, of which the victim to chronic alcoholism is such a good example.
(*c*) Other delusional states.

The salient points which mark off paranoidal from the delusional types referred to are best brought to your notice in the review submitted. That at periods of stress potentialities hitherto latent become evident, and that certain epochs tend to colour the mental outlook, are statements which have been made and are generally accepted. They are now mentioned as they appear to be the factors which determined the character of the mental symptoms in the patients to be dealt with.

The outstanding feature in the paranoidal is the systematizing of delusions. The systematization differs

from true systematized delusional insanity in that the slow evolution so typical of the true disease is not present; instead, they show the sudden onset and the rapid expansion of the delusions which Magnan[2] associates with systematization in the degenerate. The fact that the expanded delusions persist in full bloom, if one might be allowed to use a nonscientific simile, is not usual in the degenerate; it more approximates to the condition in (true) paranoia, and is a point which suggests that they as individuals belonged to what Soukhanoff[3] has termed the "ideo-obsessive constitution."

The depression present, although real, is not typically melancholic; it lacks the mental pain and concentration of ideas of melancholia, for nothing that happens around such a case escapes her notice. No matter how depressed, they never show self-accusation; they are always the wronged, and bitterly resent the injustices heaped upon them.

The five who showed this paranoidal grouping of symptoms came under institutional care during the climacteric period; they were the only illustrations from among 750 females of all ages admitted. Examples of degenerate paranoia tinged with depression at or about the climacteric are the cases which at first on superficial observation are most likely to be mistaken for paranoidal. Differences are easily brought out, however, as besides those in systematization and progress, the past records afford valuable means of differentiation; about the degenerates accounts of excesses were obtainable; the paranoidal had been well-doing.

Investigation into the histories of the paranoidal cases supports the suggestion that they belonged to the ideo-obsessive constitution. They had always been indecisive, anxious, impressionable, touchy, scrupulous, introspective, self-satisfied, and reticent. They from time to time had been obsessed by an idea or ideas, and when so obsessed showed pedantism and dogmatism. As far as could be discovered, none had ever overstepped the obvious borderland until approaching their climacteric. Four of the five suffered from functional menstrual disorders during the period elsewhere referred to as "preclimacteric."[1] That they systematized their delusions around the reproductive system tempts the hypothesis that menstrual irregularity focussed their attention on their reproductive organs, obsession followed, systematization succeeded that, and that the depression was the manifestation of epochal phenomena. In all a neuropathic family history was elicited. In one only was there mental worry apart from the menstrual before the breakdown; in her it was considerable. In another there had been transitory recent illness before being sent to the asylum. She, it may be mentioned, showed some degree of confusion. Although none are stout, all have maintained fair health.

With regard to the delusions of persecution, all systematized from the past, building on trivial incidents recalled, which on their own statements they had not attached any importance to at the time, and had not thought of for years. After the delusions had expanded rapidly there was no tendency to add from the present. The persistence of the delusional state has already been mentioned. None have shown signs of evolving notions of grandeur, neither are there symptoms of dementia. When placed under care they spoke openly of their delusions, now they write about them, but reticence in speech is marked, a form of self-control gained as the resentment against their ill-usage is strong, and ideas of public vindication still hold their place. Three are married, two came safely through the stresses of parturition and lactation, the third is childless.

The depression shown throughout has been fitful, both as to duration and intensity. The occurrence of periodic waves of emotional depression, noticeable at somewhat irregular intervals, recalls Clouston's[4] query as to whether in the periodic exacerbation of excitement in senile women there was a basis of origin in association with memory of former menstruation.

The prognosis with regard to recovery is hopeless, and their chances of returning unrecovered to family life are very poor, as the nature of their delusions, and their determination to be vindicated, precludes their liberation from institutional care.

I do not pretend to have gone into the subject in full, but systematized delusions are always interesting, and I trust that the points brought to your notice may have been of interest; and in conclusion I must thank the Medical Superintendent of Gartloch Mental Hospital (Dr. W. A. Parker) for permitting me to use the records of cases.

REFERENCES.

[1] Vera and Præsenilis Melancholia at the Female Climacteric, L. D. H. Baugh, M.B., BRITISH MEDICAL JOURNAL, September, 1908. [2] Chronic Delusional Insanity of Systematic Evolution, Magnan, translated by Drs. A. Marie and John Macpherson, *American Journal of Insanity*, vol. li, No. 4, vol. lii, Nos. 1, 2, 3. [3] Dr. Serge Soukhanoff, *Revue Neurol.*, June 30th, 1903. [4] Dr. Clouston, *Mental Disorders.*

THE VESTIBULAR APPARATUS AND THE CEREBELLUM.

By Privat-Dozent Dr. BÁRÁNY,
Vienna.

I WISH to describe two methods of examination confined to the diagnosis of cerebellar diseases. The first of these methods investigates the equilibrium of the body, the second the movements of the extremities during vestibular irritation. The disorders of equilibrium are tested by Rhomberg's test—that is, the patient has to stand with closed eyes and feet together—and we must first distinguish between (*a*) spontaneous disturbance of equilibrium, (*b*) disturbance of equilibrium experimentally produced.

(*a*) If a man has by any disease of the labyrinth a strong rotary nystagmus to the right, he will fall to the left on assuming the position for Rhomberg's test; that means he falls in the plane of the nystagmus, but opposite to the direction of the quick component of the nystagmus. If, maintaining the same position, only the head is turned 90 degrees to the left, the patient falls backward; if the head is turned 90 degrees to the right he falls forward. This is the law characteristic for the vestibular disorders of equilibrium which I have detected.

(*b*) The experimental disturbance of equilibrium can be produced in every normal man by eliciting a vestibular nystagmus through turning or syringing. There is only to say that, just as the spontaneous disturbances of equilibrium with different people are of different strength, also the experimental one with normal individuals shows great personal differences. I have seen normal men with the strongest nystagmus stand without moving, and others with slight nystagmus fall like a tree. If we produce rotary nystagmus to the right by syringing the left ear with cold water, or by turning ten times to the left with the head bent 90 degrees forward, the patient, on assuming the position for Rhomberg's test, will in falling follow the rules given under (*a*).

In cases with cerebellar disease we observe spontaneous disturbance of equilibrium which, at first sight, is just the same as in vestibular disease. But, as I have shown, there are very important differences if you examine exactly. In cerebellar disease there is: (1) no connexion between the mostly present spontaneous nystagmus and the direction of the falling. The patient may have nystagmus to the right—after the rule above given—and should fall to the left, but he really falls to the right. (2) The influence of the position of the head upon the direction of the fall is absent. Change of position of the head does not change the direction of the fall.

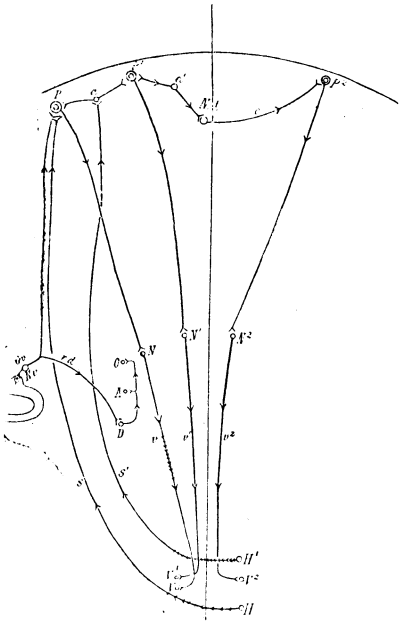
In the last year I have examined a great number of cases with cerebellar troubles, and I always tested also the experimental nystagmus and its influence upon the falling. I have found that very often syringing with cold water produced quite normal nystagmus, but the influence upon the falling was abnormal. If I syringed the left ear with cold water I got strong rotary nystagmus to the right, but nevertheless the patient did not fall to the left, but fell forward. Turning the head 90 degrees to the left side did not make him fall backward, but he still fell forward. If I can produce a definitely abnormal reaction, I make the diagnosis that a disease of the vermis of the cerebellum is present. At the last meeting of the Vienna Neurological Society I was able to present the cerebellum of a patient where alone from this behaviour I made the diagnosis tumour of the vermis of cerebellum, which was found in the dissection.

How is the behaviour of the cerebellar cases to be explained?

To understand it, we first must understand the normal. We have heard that with a certain vestibular nystagmus falling in a certain direction is combined, when the head is

erect in the ordinary position. When the head is turned, the nystagmus does not change, but the direction of fall changes—that is, other muscles of the trunk are innervated. What produces this change of innervation? It is surely not a vestibular irritation, elicited by the turning of the head, because the fall changes its direction just in the same way if the head remains quiet and the body is turned under the head. It can only be the irritation of the joints and muscles of the neck, caused by the turning of the head, which produces this change of falling. For this purpose the centripetal innervation arising from the neck muscles and joints must on some spot of the brain act together with the vestibular innervation.

It would go beyond the scope of this paper if I discussed all the possibilities which the finer anatomy of the brain and experiments on animals suggest. I will mention one possibility which seems to be the most probable. This is, that the fibres of the cerebellar lateral tract and the fibres of the nervus vestibularis come together in the cortex of the cerebellum (Cajal), and that here this influence takes place. I show a diagram which illustrates these conditions. You see the semicircular canal, and from this goes the nervus vestibularis through the vestibular ganglion, then dividing in two parts, the ramus ascendens and the ramus descendens (Kölliker). The latter goes to the Deiter's nucleus and produces the vestibular nystagmus by means of the posterior longitudinal bundle. The ramus ascendens after Ramon y Cajal enters into the cerebellum and ends round a Purkinje cell P. From there the axon of this cell goes down to a cell of the vestibular nucleus, the axon of which descends into the spinal cord (vestibular spinal path). In this way with a certain nystagmus a certain innervation of the muscles of the trunk is combined. Now



Median line.

we still have to consider the influence of the change of the position of the head; s represents a fibre of the cerebellar lateral tract which goes to the same Purkinje cell P, where the ramus ascendens of the vestibular nerve ended. This is the fibre which is innervated in the ordinary position of the head; now the head is turned. In this moment the fibre s^1 is innervated, the fibre s prohibited. The fibre s^1 goes to an association cell which brings into connexion Purkinje's cell P with P^1. You have only to imagine that the prohibition in fibre s hinders the vestibular innervation to pass through the axon of the first Purkinje cell P, and that the innervation of the association cell A makes the innervation pass to cell P^1. From here it goes down to another cell of the vestibular nucleus, and to another cell of the spinal cord. Like that, other muscles are innervated with another position of the head. If you consider how many muscles are innervated, how exactly the change of position of the head changes the position of falling, you can imagine that this mechanism is a very complicated and delicate one.

According to Nothnagel and other authors, especially Bolk, Horsley, etc, the vermis of the cerebellum has to do with the equilibrium of the body. Therefore, I believe that the cortex of the vermis of the cerebellum is the place where this mechanism is situated. Diseases of the vermis, therefore, will damage, more or less, this mechanism, and it seems that the proof for the correctness of my reasoning is given by the above-mentioned dissection. I know quite well that only a number of cases and dissections will make out how much this hypothesis is true.

If we cause a normal individual with closed eyes to stretch out his arm and touch with the forefinger an object held in front of him, and then draw back his hand and point again at the object, we find that nearly every one—even the uneducated and children—can, with a small amount of practice, point with accuracy at the object. If we turn a normal individual ten times on a turning chair to the right and stop suddenly, we observe a strong vestibular nystagmus to the left. If at this moment we repeat the "pointing experiment," the man will no longer point correctly to the examiner's finger, but point to the right side. This pointing to the side is surely due to a vestibular innervation of the arm muscles. It is only the question from where this innervation comes. Again I do not wish to tell you all the possibilities which I have discussed in a paper read before the Neurological Association in Baden-Baden, 1910. I only tell you the result of my reasonings—that this innervation comes from the cortex of the hemisphere of the cerebellum. The most important fact which made me assume this hypothesis is, that in two cases of operated healed cerebellar abscess of the right hemisphere the patient who did not show any trouble from the standpoint of the neurologist, after ten times turning to the left, did not point to the right side with the right hand (the hand of the diseased side), but pointed absolutely correctly, whereas the hand of the healthy side showed the usual error. I supposed that by the abscess the centre in the cerebellar hemisphere was destroyed, which in the normal delivers the desired innervation.

A second important fact that I have observed in fresh cases of cerebellar abscess and of probably cerebellar trauma: The patient without turning did not point correctly, but always pointed to the side of the examiner's fingers with the hand of the diseased side. I thought that this "spontaneous fault" was due to an irritation of the same centre, which if destroyed shows no influence after turning.

Another fact which made me suppose that the cerebellum is the centre for this innervation is that just in the same way as the disorders of equilibrium are influenced by the change of position of the head, also the pointing movements are influenced. If after ten times turning to the right the head of the individual is bent 90 degrees to the left shoulder, he will mostly not point to the right, but upward from the examiner's finger. If the head is bent 90 degrees to the right he will point downward. This influence is sometimes very clear, in other cases not so evident, but to explain it you must suppose exactly the same mechanism in the cortex of the hemisphere as in this of the vermis.

I have still to tell you another interesting result which I have got with this new test. If after ten times turning you let the patient point, with closed eyes, to the tip of his nose, he always will point correctly, even if he makes a very big fault in pointing to the examiner's finger. How is this possible? Also in pointing to his own nose the vestibular innervation of the arm muscles must take place. But still no mistake is made. That can be only understood if you assume that the effect of the vestibular innervation is corrected by the cortex of the cerebrum. But the cerebrum can only correct the cerebellar innervation if this innervation is known to it. It seems to me that the brachium conjunctivum which travels from the cortex of the cerebellum through the red nucleus to the cortex of the cerebrum may have this function. I draw the further conclusion that if this is true, a case with hemiathetosis, where, according to our present view, the brachium conjunctivum is interrupted, the patient should not be able to correct the cerebellar innervation, and should also, in pointing to his own nose, make an error, as in pointing to the examiner's finger. I was so fortunate as immediately to find such a case among the rich material of our Vienna clinics, and my supposition was confirmed.

Whereas before the turning the patient pointed correctly to the examiner's finger and his own nose, after the turning he made the usual fault in pointing to the finger, and he could not reach correctly the nose with his finger, but touched it always with the back of the hand.

DISCUSSION.

Dr. STANLEY BARNES (Birmingham) asked if Dr. Bárány had yet come to any conclusion as to the causation of the tremor in disseminated sclerosis. He expressed the opinion that it was due to diseases of the fibres connecting the cerebellum and the cerebrum, but had had no opportunity of applying Dr. Bárány's new tests in investigating which of these tracts was affected. He instanced a case of cerebral (posterior parietal) tumour with symptoms characteristic of lateral cerebellar tumour, as showing the need for the adoption of fresh tests to indicate the position of disease in conditions where equilibration was disturbed or ataxia was present.

Dr. DRAPES (Enniscorthy) gave some details of a case under his care which had an intimate bearing on the facts which formed the subject of Dr. Bárány's address, and in which the diagnosis presented exceptional difficulty. It was that of a lady who, some three years ago, without any definite prodromata, fell into a state of unconsciousness which continued for two or three hours. Since then she had had two similar attacks at long intervals, and had suffered from headache and giddiness at variable intervals. Eight months ago the giddiness had become very distressing, and on examination she was found to have a moderate amount of hypermetropia, and also slight deafness and tinnitus. There was no vomiting and no optic neuritis. The visual defect was corrected by suitable lenses, and she had no attack of giddiness for three months. She then began to suffer from it again, and also from headache. The plantar reflex in both feet was found to be *nil*, and the knee reflex was absent on both sides. These symptoms pointed to central disease, but in order to eliminate other possible causes a consultation with an aural specialist was held. On testing the patient in a revolving chair she was found to have rotatory nystagmus. The test caused intense giddiness and bewilderment. The consultant was of opinion that the symptoms were due, mainly if not wholly, to labyrinthine trouble, possibly depending on the action of intestinal toxins, and advised injections of pilocarpin and sour-milk treatment. Result: no attack of giddiness or headache for the last three months and great improvement in the general health. But the same condition of the reflexes continued, and it was a question still whether there might not be cerebral or cerebellar disease coexisting with aural trouble. It was in a case of this kind that the exquisite tests detailed by Dr. Bárány would be of the highest value in arriving at a differential diagnosis.

Dr. ALEXANDER BRUCE (Edinburgh) said that knowledge of the anatomical connexions and physiological associations of the nervous mechanism of the vestibular system was still in its infancy. Dr. Bárány had shown that what was already known might be justly utilized for diagnostic purposes both by the otologist and the neurologist. He had shown what might be done in the localization of tumour and abscess in the cerebellum. His method of investigation would seem likely to be of special value in the early diagnosis of insular sclerosis when it invaded the pons and medulla before the appearance of the classical symptoms of nystagmus, intention tremor, etc.

The PRESIDENT said he had great difficulty in eliminating the cortical factor from consideration. He had seen many cases of loss of co-ordination, giddiness, nystagmus, and other symptoms which might have pointed to vestibular or cerebellar disease. In these cases, however, the symptoms had proved to be due to cortical irritative lesions or implications of the cranial nerves at the base of the brain. Dr. Bárány's paper was of very great value to neurology, and his references to traumatism were of particular interest, inasmuch as the questions of nervous disorders which had arisen in consequence of the Workmen's Compensation Act had increased enormously in number. They were as yet in the infancy of their knowledge as to the direct and remote mental and physical results of traumatism.

Dr. BÁRÁNY, in reply to Dr. Stanley Barnes, said that he had not been able to come to any definite conclusion with regard to the causation of the tremor of multiple sclerosis. Concerning Rhomberg's test, it would be very easy, by making this test more difficult, to discover still more patients who would fail to carry it out, but in his opinion only such tests should be employed which a normal person could easily perform. Hence the making of Rhomberg's test more difficult could not be recommended. The speaker stated also that he was not sure that the vertigo which was considered cortical was really cortical, the vestibular apparatus being so exceedingly sensitive that very slight changes of the circulation in a region of the vestibular nuclei might possibly produce such attacks. They were, as a matter of fact, at the beginning of their knowledge of traumatic cases, and he could only hope that fresh methods would be discovered which would more and more elucidate the causation and nature of the so-called traumatic neuroses—a category which would finally disappear by the relegation of cases of this kind to classes of a more strictly scientific character. He trusted that the methods which he had described might form one step in this direction.

ON THE TEACHING OF PSYCHIATRY.

By D. G. THOMSON, M.D.Edin.,
Medical Superintendent, Norfolk County Asylum.

My thesis is that the various universities and other qualifying bodies should establish post graduate curricula and diplomas in psychiatry or psychological medicine. It is surely a striking fact that, while various special branches of medical practice have such curricula and diplomas, the speciality in which we practise has none. In public health, tropical medicine, naval and military surgery, and hygiene, there are special recognized post-graduate courses of study and diplomas and commissions, without having undergone and obtained which no one would think nowadays of practising those specialties.

It would be ungracious not to allude to the fact that certain qualifying bodies have made some move in this direction—for example, the Universities of London and Dublin permit their Bachelor graduates to proceed to the Doctorate in psychological medicine; that for the M.B. degree in Edinburgh and in some other universities it is compulsory to attend a course of lectures in psychological medicine; and that the Medico-Psychological Association has instituted an examination and certificate in psychological medicine; but while the half-loaf provided by those purveyors of intellectual food may be better than no bread, the fact remains that there is no definite post-graduate curriculum and diploma open to a young medical man who wishes to equip himself to deal with the problems of the pathology and treatment of mental disease which will face him from the moment he begins lunacy practice.

The constantly increasing number and extent of the subjects comprised in the five years' medical curriculum—a load on the student's back already almost more than can be borne—renders anything more than a general summary of each subject impossible; therefore, if a young graduate wishes to engage in any special department of medicine, he must of necessity make a further study of that speciality.

Teaching authorities have already recognized this as regards public health and other specialities; in the first-named the law even demands it, and so we have post-graduate curricula in public health and tropical medicine at the medical schools. But, under present conditions, a young graduate is pitchforked into the midst of the most bewildering and difficult of all specialities without any special training whatever. I cannot help believing that if the public realized that the some 130,000 insane of this country are given over to the care of medical men for whom no special teaching and qualifications are provided by the teaching bodies, or demanded by those who appoint them to the medical charge of the insane, there would be a legitimate outcry.

Years ago I remember eloquent appeals being made by Sir James Crichton-Browne, Dr. Clouston, and others for the promotion of the medical spirit in asylums; there is as much, and more, need to-day for such an appeal, but there will be no adequate response to that appeal till we have joining the junior ranks of our speciality young men specially and highly trained for the work. We want the flower of the young graduates to enter this difficult speciality, and these, I hear, are actually advised not to enter it as it is at present constituted.

I do not for one moment undervalue the splendid work which has been done, and is being done, in asylums, both in scientific research and in medical and administrative work, but it is as a drop in the ocean to what should be done, and I doubt if 10 per cent. of us—and I include myself among the remaining 90 per cent—are capable of carrying out modern clinical and pathological research necessary to the study of a case of acute mental disorder.

It appears to me that the time is opportune for such a movement; recent researches in general medicine, the infinite possibilities arising out of the new sciences of bacteriology, haematology, bio-chemistry, inspire in us fresh hope that if we could only get our ranks recruited by trained and able young workers, the great fields for work in our asylums would be harvested in a way, and with results, which at present are impossible. However, I need not labour this point further; I think it is obvious to all of you. I should rather employ the remaining time at my disposal in considering some of the objections raised to the scheme. No objection has been, or can be, raised to this proposal on principle; the arguments against it are purely utilitarian, one I am often met with being that the difficulty of obtaining assistant medical officers in our public and private asylums is already sufficiently great, and that if a post-graduate course and diploma become necessary qualifications, assistant medical officers will become unobtainable. At first sight this might appear so, but when this subject was first discussed several speakers were of opinion that so far from this fear being justified, it is more likely to be the means of increasing the supply of candidates. It might be stated here that the more highly qualified men might expect higher emoluments, although I would point out that there seems to be no dearth of candidates possessed of Public Health diplomas for school medical officerships, where the prospects are much poorer, where the emoluments are very similar to those of assistant medical officers entering the asylum service—namely, £250 per annum.

Another objection put forward is that under present conditions asylum medical officers are overburdened with more or less necessary routine and secretarial duties, that they have no time for purely medical, clinical, and pathological work among the recent and acute cases. This is true to some extent, yet the same applies to public health, army medical, or any other branch of official and administrative medical work; moreover, there is a decided trend in the direction of alleviating this state of affairs in the shape of separation of the acute and recent cases either in separate mental hospitals or in wholly separate departments of the general asylums, whereby much more skilled attention can be given to such cases, correspondingly less medical attention being necessary for the chronic, incurable insane. Such trend is seen in Dr. Maudsley's munificent, but, I fear, not sufficiently appreciated, gift to the London County Council for the establishment of a mental hospital, and also in the findings of the Royal Commission on the Feeble-minded.

Another utilitarian objection is that committees of visitors would look askance at the scheme, involving as it would increased cost, to some extent in more medical officers, and to a greater extent in the provision of facilities for clinical and research work; but this can be met either by a change in the law regulating appointments to asylums such as obtains in the Public Health Act, or by educating committees, either individually or collectively, through the county councils to an appreciation of the advantages to the insane, and the probability of a higher recovery-rate by a higher standard of ability in their medical officers.

Another objection—that of the pessimists, who, like the poor, we always have with us—is to the effect that insanity is a hopeless mystery, unknown and unknowable, like religion; that the curable cases recover whatever we do, or do not do, to them, and incurables do not; that "we are very well as we are," and so on and so forth. This position I can sympathize with and understand, while I have no patience or common ground with those who pretend that insanity is a bodily disease, and yet see no advantage in having a preliminary post-graduate training for those whose life's business it will be to investigate and treat this disease.

And now briefly to report progress in this movement. My original paper was, as I have stated, read in April, 1908, at the annual meeting in London the matter was brought up at a meeting of the Educational Committee of the Medico-Psychological Association, and a subcommittee was appointed to consider the matter and report; this subcommitteee to consist of Drs. McDowall (Chairman), Bedford-Pierce, Bevan-Lewis, Robert Jones, David Orr, Percy Smith, D. G. Thomson, and Whitcomb. Drs. Mercier, Rows, and Stoddart were subsequently added to the subcommittee. This subcommittee held meetings at York, Leeds, Norwich, Cambridge, Leicester, and London, and submitted a report to the annual meeting of the Medico-Psychological Association at Leeds in July, 1909, in which it strongly recommended the institution of a post graduate curriculum and diploma in psychiatry, and that the universities and medical schools be approached with a view to their instituting the same.

This was approved by the association, and the President was authorized to send the following letter to the universities and medical schools of Great Britain and Ireland.

Medico-Psychological Association of Great Britain and Ireland,
11, Chandos Street,
London, W.,
August, 1909.

Sir,—I am instructed by the Council of the Medico-Psychological Association of Great Britain and Ireland to approach the Universities and other Examining bodies on the subject of post-graduate instruction in Psychiatry.

It has long been felt by those most intimate with the subject that there is in this country no adequate systematic instruction in Psychiatry. The evils of this neglect become year by year more and more manifest. This Association is impressed with the urgent necessity for post-graduate teaching in Psychiatry in Medical Schools and for the granting of a special diploma to candidates after examination, as has already been done with such conspicuous success in Public Health and Tropical Medicine. The position of Psychiatry as a branch of Medicine is unsatisfactory; it is not properly affiliated to other departments of medicine, to their mutual detriment; and under present conditions cannot make full use of those modern methods of research which have resulted in such advances in general medicine.

Young medical men on their appointment as medical officers to asylums find themselves face to face with work and problems of which they have had no previous knowledge, and in preparation for which they can obtain no systematic and scientific training or teaching. As is well known, lectures on Psychological Medicine, and pathological laboratories have been established here and there, and, in one or two Universities, Chairs of Experimental Psychology have been founded; but there is no systematic course of instruction and no recognized diploma at the end of such course. It is submitted that the time has now arrived when such a course and diploma should be established in the principal medical schools of this country, and a diploma in the subject should be instituted by the examining bodies. My Association is of opinion that the institution of a diploma would impose a high standard of acquirement in the officers of asylums, would stimulate the scientific study of insanity, and would have an effect in widening and deepening our knowledge of the subject, comparable with the effect produced in Public Health and Tropical Diseases by the institution of diplomas in these subjects.

In this letter it is unnecessary to enter into details as to the time required for this post-graduate work, and the subjects to be studied; probably each University and examining body will form its own views on these points. I may say, however, that my Association considers that the minimum period should be one year; that provisionally the subjects should be divided into (*a*) compulsory and (*b*) optional; that in the former should be included (1) Anatomy, Physiology, and Pathology of the Nervous System; (2) Psychology, Normal and Morbid; (3) Clinical Pathology; (4) Clinical Neurology; (5) Psychiatry, Systematic, Clinical and Medico-Legal. The optional subjects suggested are (1) Experimental Psychology; (2) Bio-Chemistry; (3) Bacteriology; (4) Comparative Anatomy and Physiology of the Nervous System; (5) Eugenics. It is suggested that only one optional subject be required of candidates. The Council of my Association respectfully asks that its proposals may receive earnest consideration, so that in the near future it may be possible to place the teaching of Psychiatry on a sound scientific basis, and so bring it into line with other special departments of Medicine in this country.

I am, Sir,
Your obedient servant,
CHAS. MERCIER,
President (1908-9).

The response to this letter has been gratifying, the Faculty of Medicine of Edinburgh University and of the Victoria University, Manchester, having already gone so far as to recommend the Senate to adopt the recommendation, and regulations for a curriculum and diploma in psychiatry have been approved. The Universities of Cambridge and Durham have nearly completed their arrangements.

During the past year the subcommittee which was re-appointed has held further meetings to formulate a model syllabus of post-graduate study and to consider further matters connected with the movement, and to discuss how best to obtain the support and influence of the Commissioners in Lunacy, the county councils, and the committees of visitors of public asylums and other bodies and persons concerned in asylum administration.

Such, then, is the movement and its progress for the institution of a post-graduate curriculum and diploma in psychiatry; and with your permission, Mr. President, I would like to propose the following resolution:

> That this meeting of the Section of Psychological Medicine and Neurology, assembled at the annual meeting of the British Medical Association in London, 1910, believing that it would tend to advance our knowledge of the pathology and treatment of mental diseases, strongly approves of, and recommends the institution of, a post-graduate course or curriculum and a diploma in psychiatry; and, further, that this resolution be brought before the Council of the British Medical Association.

DISCUSSION.

Dr. Cole (London) spoke of the state of post-graduate study as it existed at present in London in this special branch of medicine. Occasional lectures were delivered and clinical demonstrations were given, but no definite course covering the whole ground of psychiatry was available for the qualified medical man. He had to visit different asylums, hospitals, and laboratories to obtain his knowledge, and the certificate of the Medico-Psychological Association had not carried the weight that it deserved in the competition for an appointment. He considered that a university diploma involving a recognized course of study would lead to far-reaching beneficial results, both as regards the better training of the asylum medical officer and the more scientific treatment of the insane.

Dr. Pasmore (Croydon) agreed that something was necessary as regards a systematic training of psychiatry. One saw every day the appalling ignorance of the general practitioner in signing certificates. He thought, however, that the curriculum was too elaborate, and that in the modern method of training a good deal might be left out and the curriculum made to consist of elementary psychology and psychiatry. He felt, however, that some systematic training was necessary, and he had much pleasure in supporting the resolution.

Dr. J. F. Briscoe (Alton) supported Dr. D. G. Thomson on a post-graduate curriculum and diploma in psychological medicine, believing that a vast number of general practitioners were ignorant of psychological medicine, and that such a diploma was necessary to insure adequate skill and knowledge on the subject from those who practised psychiatry.

Dr. Shore (London), after expressing appreciation of Dr. Thomson's paper, remarked that he had been for about twenty years in general practice. He wished to take the point of the attitude of so many general practitioners to melancholia—for example, the lack of appreciation of the fact that the melancholic was a potential suicide, and that the burden of proof lay on the medical adviser to show that a given case was not suicidal. As things were, the general practitioner could not know everything, and it would be well to recognize the limitations in certain mental cases, and call in a medical man having special experience of mental diseases. As regards teaching psychology, Dr. Shore referred to the teaching of psychology at King's College, London, in the course for the B.D., as an illustration of how not to do it. It would be better if a simpler textbook were chosen, and the class carried on step by step, as it were. There seemed to be a lack of present security that the students there were able to follow the subject of psychology satisfactorily.

Dr. Seymour Tuke (London) wished to join in every way with Dr. Thomson and the proposals embodied in his paper. There seemed to be two points in the proposal for a diploma—first and foremost, the benefit to the insane, and next the benefit to the profession. There could be no doubt that the insane were not benefiting by the present condition of things. For the time, at any rate, knowledge of neurology seemed in some cases preferred to actual clinical knowledge of mental disorder, and if this proposed diploma could bring together in one individual a good knowledge of the two (for they ought to go hand in hand), they might be spared many curious experiences. The profession must benefit, for whether the possessor of the diploma made insanity his special study or not, he would be there to be consulted by men whose knowledge was less. The speaker was happy to see so many young medical men—medical officers of asylums in most cases—combining most successfully a knowledge of the whole subject, and showing that a combination such as was looked forward to by the originators of the proposal was possible. If there was any fault to find with the details, it was with the apparent preponderance of the psychological and neurological knowledge required, compared with the knowledge of clinical and other knowledge of mental disorders, but doubtless this was more imaginary than real, for though occupying a small space in the syllabus the ground covered was really large.

The resolution was then adopted.

DISCUSSION ON THE TREATMENT OF TABES DORSALIS.

OPENING PAPER.

By J. S. Risien Russell, M.D., F.R.C.P.,

Professor of Clinical Medicine and Physician to University College Hospital; and Physician to the National Hospital for the Paralysed and Epileptic, Queen Square, London.

The only rational treatment of a disease is that which is based on a correct view of its etiology and pathology, so that a discussion on the treatment of tabes carries with it the necessity of a clear conception of the cause of the malady and the morbid process responsible for the symptoms.

The vexed question as to whether tabes is or is not of syphilitic origin continues to exercise the minds of not a few, despite the evidence which seems to some of us well-nigh overwhelmingly in favour of the view that syphilis is the important etiological factor, without which there would be no such disease as tabes. In my own experience it has so rarely happened that I have not been able to obtain a definite history of syphilis in patients who have come under my observation suffering from tabes, that I find it impossible to convince myself that any view of the genesis of the disease is correct other than that which ascribes the affection to syphilis. Nevertheless, it has to be admitted that isolated instances are met with from time to time in which the clinical picture precludes the possibility of any opinion other than that the patient is suffering from tabes; and yet it is impossible to obtain a history of the primary syphilitic infection, even where the patient wishes to be helpful and has no reason to conceal the truth. Here it is that the modern tests for syphilis may be appealed to in reason; but no one who has had much experience of these tests is surprised to find that while in some instances they supply us with the positive information that we saw reason to expect, in others the results are negative, and we are, accordingly, no nearer to the solution of the problem that is before us. Such negative results cannot, however, allow us to abandon a position that has so much to support it. For it must have been the experience of some at least of those who are to take part in this discussion that, with the unmistakable signs of tabes and the certain history of syphilis, these tests have, nevertheless, supplied us with negative instead of positive information in some cases.

Although I hold no special brief for the defence of the position that tabes is due to syphilis, nevertheless I am compelled to enter on these considerations in view of what I have to say in regard to the treatment of the disease; and I find it impossible to make myself believe that because it is the experience of most of us that from time to time we are confronted with what appear to be otherwise typical instances of the affection, and we are unable to prove syphilis, we are therefore to abandon the position that syphilis is *the* etiological factor that is to be blamed in the vast majority of cases of the disease.

Whether or not such negative cases should be treated for syphilis is open to question, so that I do not propose, if possible, to allow them to obscure the main issue that is before us. I say, without the slightest fear of legitimate contradiction, that no one in this room with any real knowledge of the disease is in a position to deny that the majority of those who are the subjects of tabes have had syphilis. This raises the important question as to whether, with this knowledge before us, we ought to have these patients treated for syphilis or not. I make no hesitation in answering that, with certain reservations, it is our duty to do so; and, moreover, that it is unfair to our patients to allow them to go untreated for syphilis when they consult us in consequence of tabes, especially when we are able to determine a positive history of the primary infection which has led to this more remote consequence.

One of the reasons that I accepted with such alacrity the invitation to open the discussion was the hope that this would prove an opportunity not to be lost of waking the profession in this country, and of making practitioners realize the importance of antisyphilitic treatment in tabes, and what is meant when we speak of thorough treatment of the disease.

It commonly happens that I see patients suffering from this disease who have been under treatment for years, and in whom the treatment for syphillis has been altogether neglected or so imperfectly carried out as to be quite useless. Moreover, it is often difficult to persuade practitioners of the need and efficacy of antisyphilitic treatment, in view of the fact that some of our authorities have laid it down that, in spite of the syphilitic origin of the disease, antisyphilitic treatment is useless, and is even calculated to do harm. In reality nothing could be further from the truth, always observing certain reservations in this connexion.

I do not propose to enter into any lengthy discussion of the merits and demerits of one method of antisyphilitic treatment as opposed to another, but must content myself with saying that, in spite of all that can be urged against it, I have seen the best results from inunction, as carried out at Aachen, either alone or supplemented by intramuscular injections of one of the soluble salts of mercury. It would not be right for me to contrast this method of treatment with that in which intramuscular injections alone are relied on, for the reason that the inunction treatment has, as a rule, yielded such satisfactory results that I have not been inclined to depart from it without considerable reluctance, with the natural result that my experience of the injection treatment is small as compared with the other method. My reluctance to abandon inunction for the injection treatment has been accentuated by the fact that I have met with an occasional case in which the patient previously was treated by injection without notable result, and in which, nevertheless, success has attended inunction subsequently carried out.

Whatever room for question there may be as to the value of inunction, as contrasted with injection treatment, I can admit of no such uncertainty with regard to the inferiority—indeed, I might almost say inefficacy—of mercurial treatment by the mouth, as contrasted with inunction, and it may be, injection. For I repeatedly see patients in whom mercury has been given by the mouth for long periods without any benefit, and in whom, nevertheless, subsequent treatment by inunction has yielded excellent results. All that I would ask, therefore, is that the patient should be thoroughly treated by inunction or injection, and that valuable time should not be thrown away in pills or potions administered by the mouth.

The details of the inunction treatment I propose to leave to others who are to take part in this discussion; but I may, perhaps, be allowed to say that I advise patients to go to Aachen for their treatment when this is possible, and that when there are reasons which prevent this, I try to secure for them the treatment in this country on as nearly as possible the same lines as obtain at Aix-la-Chapelle. I mention this for the reason that much of what is spoken of as inunction treatment in this country is little short of a farce, and that many of the patients that are so treated might just as well smear themselves with lanoline or vaseline as with the mercurial ointment which they are advised to rub into this or that part of their bodies. Moreover, that even where someone is secured to rub in the ointment, this is done in such a perfunctory manner as to be practically useless.

When there is nothing to contraindicate this, I usually advise the patient to undergo a course of 100 rubbings, to be followed in three to six months, as is found best, by another course of not less than 50 rubbings, which should be repeated every six months for about three years if nothing happens that appears to contraindicate a course of treatment of this duration. When intramuscular injections are combined with inunction, an injection is given once a week, while the inunction is persevered with for the remaining six days in the week. The important adjuncts that are called for in the treatment I must leave to those who are to follow me in the discussion.

It has not been my experience to meet with any of the bad cases of salivation which have been ascribed to this method of treatment, so that I can only suppose that there must be some radical difference between the way in which the mercury has been exhibited by those who have recorded such untoward results and the plan which is in vogue at Aix-la-Chapelle.

The next question that arises is as to what cases of tabes should be submitted to the treatment. My own experience does not in the least support the view that appears to prevail in this country, and which supposes that it is useless to submit a patient suffering from tabes to antisyphilitic treatment, except when the tabes follows closely on the primary syphilitic infection. I have met with notable instances in which the primary infection has been remote, and in which, nevertheless, the patient has improved under antisyphilitic treatment in a manner that has made it impossible for me to believe otherwise than that the result has been due to the treatment. The not uncommon history of tabes following on a primary infection ten, fifteen, or even twenty years before is, to my mind, no contraindication to the antisyphilitic treatment, provided there are no other reasons which make it inadvisable to submit the patient to a cure of the kind.

Another important question that arises in this connexion is as to how far our advice in the matter should be influenced by considerations as to whether the patient was treated for the primary syphilis; and if so, with what degree of efficiency. I find it difficult to make any dogmatic statement on this point, for the reason that it is so difficult to estimate how thorough the treatment of the primary disease has been. As is to be supposed, the majority of the patients I see suffering from tabes have had their syphilis treated according to the method that hitherto prevailed in this country, and thus they had been put through courses of mercury by the mouth for varying periods. In addition to this, no small proportion of the cases have either not been treated at all or but for a few months, so that they may practically be regarded as having been untreated.

As to whether a patient will develop tabes or not, does not appear to be in the least influenced by the severity of the primary infection, or the subsequent secondary manifestations of the disease, for, in reality, a large proportion of the tabetics are drawn from the class of case in which the primary infection and secondary manifestations have been so slight as to lead to very imperfect treatment. Even if we feel that it is possible to conclude that the treatment of the primary syphilis has been thorough, however, it would seem to me a logical conclusion at which to arrive that the fact that tabes has nevertheless developed is evidence that the syphilitic taint has not been completely removed, and that, accordingly, further antisyphilitic treatment is called for, if we accept the position that tabes is syphilitic in origin, in spite of the fact that there are those who contend that antisyphilitic treatment of cases that have had their primary syphilis well treated is practically useless.

Intimately related to this aspect of our subject is the view of those who consider that syphilis is an incurable disease, and that accordingly it becomes necessary to combat the virus of the disease from time to time by the antidote that has proved most effective. If this view is to be accepted, then there would be no room for question as to what should be done in the case of a patient suffering from tabes, no matter how thorough the treatment of his syphilis has been.

Another consideration that forces itself on us at this stage of our inquiry, and which demands our attention, is

the outcome of the investigations of Krön, who finds that tabes occurs at increasingly shorter intervals after the primary attack of syphilis, the longer the antisyphilitic treatment has been kept up.

This naturally brings us face to face with the suggestion that has not been wanting in supporters, that the mercury used in the treatment of syphilis, rather than the syphilis itself, is responsible for the affection of the nervous system. No better argument can be adduced to controvert this view than the fact that cases are not wanting in which the patient's syphilis has gone untreated by mercury or any other drug, and in which, nevertheless, tabes has developed.

This would appear to be as favourable an opportunity as any for considering the view that tabes is not due to the syphilitic virus itself, but rather to the antibodies which result in the blood as a consequence of the syphilis. The question that thus arises is as to whether we may reasonably suppose that antisyphilitic treatment can have any effect under such circumstances.

If we are justified in considering that tabes is due to syphilis, the question as to whether or not antisyphilitic treatment may reasonably be expected to do good is intimately associated with what view of the pathology of the affection finds acceptance. For it is reasonable to suppose that mercury will be more likely to have an effect on the lesion if it be of the nature of a meningitis with interstitial inflammatory affection of the neuroglial tissue than if we are to regard it as a neuronal degeneration, primary in origin, and not secondary to affection of the meninges and interstitial tissue. However this may be, the fact remains that patients suffering from tabes have, to my knowledge, improved to such an extent that even their knee-jerks have returned under mercurial treatment, and, in the experience of others, not only have the knee-jerks and ankle-jerks returned, but even the pupil reflex to light, which had been lost, has been re-established. With such evidence before us, it is impossible to believe otherwise than that, whatever the true pathological changes which occur in the early stages of the disease, mercury has a potent influence for good on the lesions of tabes, for I must decline to accept the view of those who maintain that the cases in which good results from treatment by mercury have been obtained are cases of pseudo tabes due to syphilis, and not the genuine disease with which we are concerned to-day.

DISCUSSION.

Dr. David Ferrier, F.R.S., said: It will, I think, be generally admitted that in proportion to the ignorance which prevails as to the etiology and pathology of a disease, its natural history and course, and in proportion to the imperfection of its differentiation from other maladies, the wider the scope for blind empiricism, vaunted specifics, and quackery of all kinds; whereas the more precise our knowledge on these points, the more we recognize the limits that are set to our therapeutic measures. The therapy of tabes hitherto illustrates these propositions in a remarkable manner.

The rational treatment of tabes must depend on its intimate pathology and its cause. On these points we have learnt much in recent years. I would take the year 1875—the date of the publication of Fournier's first paper on the relation between syphilis and tabes—as the starting-point of our knowledge of the true etiology of tabes and its rational treatment.

The causal relationship between syphilis and tabes indicated by Fournier, at first disputed, has steadily gained acceptance, and syphilis is now admitted by all as the most frequent antecedent of tabes; while every day adds to the list of those who believe, as I do, that not only is syphilis the most frequent antecedent, but that without syphilis, whatever other causes may be co-operative, there is no tabes. I need not here repeat the arguments on which I base this opinion, as I have elsewhere detailed them at length (Lumleian Lectures on Tabes, 1906). The intervening years have only added confirmation of the views I have there expressed.

Next, as to the nature of the process itself. This has been conclusively shown to be a degenerative atrophy, more particularly of the intramedullary tracts of the spinal sensory protoneurone, the so-called exogenous tracts of the posterior columns of the spinal cord. The process is primarily parenchymatous; there is no active hyperplasia of the neuroglia, and such sclerosis as ultimately ensues is only that which occurs in any organ consecutive to the degeneration of its true parenchyma. The tabetic degeneration has none of the features usually considered characteristic of tertiary syphilitic lesions. There are no cellular infiltrations or gummatous changes, and the ordinary antisyphilitic remedies have, as is generally acknowledged, little or none of the striking influence that they exert on syphilitic lesions of the usually recognized type. Hence, it is customary to regard the tabetic degeneration as parasyphilitic—that is, of syphilitic origin—but not syphilitic proper in the ordinary sense in which we use the term. It is supposed that the degeneration is the result of a toxin developed by the living syphilitic virus, but as to its nature and mode of origin we have yet much to learn.

If these views as to the pathology of the tabetic degeneration are correct, it seems hopeless to expect that by any means in our power we can restore *ad integrum* tracts or cells which have undergone degenerative atrophy. I know of no such regenerative process in the central nervous structures. Once gone they are gone for ever.

It follows, therefore, that an *anatomical* cure of tabes (as Grasset terms it) is an impossibility. Hence, medication and measures with a view to the absorption of active proliferations, vascular and gummatous processes which do not exist, are of no avail, and may be worse than useless.

I would classify the principles of the treatment of tabes as follows:

1. Prophylactic measures.
2. Measures calculated to arrest further degeneration.
3. Measures calculated to alleviate distressing symptoms; and—
4. Such as tend to compensate for the degeneration of the tracts which are essential to motor co-ordination.

1. As to prophylaxis, I believe that the efficient treatment of *early* syphilis offers the most hopeful prospect of success in combating tabes. In this disease *par excellence* prevention is better than cure. The investigations of Fournier have shown that the cases of tabes diminish in proportion to the effective treatment of the antecedent syphilis. I quote the following from the *Gazette Hebdomadaire*, December 19th, 1891. An examination of 321 cases of tabes showed that—

1. Sixteen cases had had no treatment of any kind.
2. Eight cases had had KI only, and no mercury.
3. Of those treated with mercury, or more often a combination of mercury and KI,

70	were treated	from a few days to	two months
108	,,	,,	three to six months
51	,,	,,	seven to twelve months
23	,,	,,	about twelve months
20	,,	,,	from one to two years
5	,,	,,	from two to three years
6	,,	,,	for three years
2	,,	,,	for four years

These statistics absolutely disprove the tabetogenic effect which some have attributed to mercurial treatment, and strongly favour the view that a still more thorough and effective treatment might prevent the development of tabes altogether. It must be admitted, however, that, in spite of what most syphilologists would consider satisfactory treatment, tabes may develop, just as some syphilitic lesions prove refractory to the most energetic antisyphilitic treatment. But this leads to the consideration of what is effective treatment, and what test there is, if any, of the complete eradication of the syphilitic virus.

So much mercury by the mouth, so many intramuscular injections, so many inunctions, or so many "cures," are only rule-of-thumb guides, and give us no definite information as to what they have actually accomplished. Nothing better, however, was possible until the great discovery of the Wassermann reaction. In this, though there may be some questions still unsolved as to its exact significance, we appear to have a precise test of the existence or not of a specific virus; and the treatment of syphilis should be continued till the positive reaction is converted into a permanently negative one, as determined by repeated examinations, and, if necessary, antispecific treatment, extending, it may be, over a long series of years. Seeing that only a relatively small percentage of those infected

by syphilis subsequently become tabetic (1 to 5 per cent.), this may seem an excessive precaution, but there are few, I should imagine, who would hesitate between this and the risk of becoming tabetic.

Hitherto our most effective antidote to the syphilitic virus has been mercury in some form, administered either by the mouth, by intramuscular injection, or by inunction. I will leave to syphilologists to determine which of these is the most successful. Something may be said for and against each, and perhaps individual cases may necessitate variations or alterations, according to the conditions that prevail.

But it is undoubted that mercury may fail to abolish the positive Wassermann reaction; and this occurs more particularly in the later stages of syphilis, and in the so-called latent stage, which may exist for many years (thirty to forty) without any active manifestations whatever (Bruck).

The most successful results are attained in the primary stage, when the presence of spirochaetes has been verified, even before the positive reaction has declared itself. Therefore, an early and thorough antispecific treatment is indicated in all cases of syphilis, if we would wish to prevent after-effects. It is a question whether the combination of mercury with arsenic, as in Lambkin's atoxylate of mercury, or the *therapia sterilisans magna* by the dioxy-diamido-arseno-benzol of Ehrlich, which has been recently so much extolled in Germany, will prove more effective than mercury alone, and without deleterious effects, such as those caused by atoxyl.

Time will show, and if our anticipations and hopes should be realized, we shall have in Ehrlich's remedy one of the greatest triumphs of the day in rational therapeutics. For the present, however, we had better "wait and see."

2. When tabes has actually declared itself, the uniform presence of the positive Wassermann reaction in the serum, and frequently in the cerebro-spinal fluid, is in itself a proof of the syphilitic origin of the disease, and naturally indicates the advisability of antispecific treatment, especially if this has not previously been carried out. And there is, I think, little doubt that in many cases mercurial treatment is not only well borne, but may cause a distinct alleviation of the symptoms. But whether it ever converts the positive into a negative Wassermann reaction, and finally stays the further progress of the disease, is a question. My own experience, so far as it has gone in this relation, has been rather unfavourable, and the positive reaction in the serum has resisted the most energetic mercurial inunction. And I should not be inclined to push mercurial treatment unduly, for I have seen, as the result, such anaemia and lowered vitality as have, I think, rather favoured than retarded the progress of the disease. But one is not without hope that some modification of the mercurial or arsenical treatment, or other agent, may yet be found which will successfully neutralize the toxin which is causing such havoc in the nervous system.

I have myself seen no good results from nitrate of silver, or most of the drugs which have been at various times recommended and employed. I have for many years been in the habit of prescribing a combination of arsenic, mercury, and gold, in the form of a triple bromide of each (sold under the name of hydraurum; Bell and Croyden). This remedy, given in 10-minim doses ($\frac{1}{32}$ grain of each salt) three times a day continuously (with occasional intermissions) for some years, has, I think, been productive of beneficial results in many cases. I am well aware how difficult it is to establish the efficacy of any particular drug; and I am bound to say that in my practice I have thought more of the interests of my patient than of any therapeutical experiments, and have, therefore, not neglected anything that might conduce to his well-being. But my clinical experience would lead me to believe that many of my patients have owed their relative recovery and their continued ability to transact all the duties of life largely to the remedy I have mentioned. As I am quoting from the experience of years back, I have not investigated the condition of the blood and cerebro-spinal fluid before and after treatment, as I should now think necessary. In one recent case, however, after several years' treatment, though the patient has much improved, and is able to perform all his duties (that of schoolmaster) in comparative comfort, the Wassermann reaction is still positive in the blood and cerebro-spinal fluid, and there is a moderate degree of lymphocytosis. In another the Wassermann reaction is negative.

I have seen the knee-jerks return in one case after two years hydraurum treatment, but I cannot say that I have ever seen the disappearance of the Argyll Robertson reaction of the pupil when it has once set in. Yet though an anatomical cure of tabes is practically impossible, one might with justice speak of a *clinical* cure in many cases. And it is a fact that, notwithstanding absence of knee-jerks and the presence of the Argyll Robertson pupil, the patient may be neither ataxic nor exhibit any obvious tabetic symptoms.

A clinical cure should be considered possible in every case of incipient tabes, and it would, in my opinion, be decidedly unwise to dash any patient's hope of this desirable result in his own case. Along with medicinal remedies one would naturally, in the treatment of tabes, prescribe such hygienic and other measures as are calculated to maintain the general health and nutrition, and, above all, to guard against or forbid such modes of life as we know from clinical and other grounds favour the degeneration of the centripetal spinal tracts, more particularly over-exertion, sexual excess, etc. The experiments of Edinger and Helbing as to the effects of over-exertion in debilitated states of the system are of great importance in this relation. And if there be any truth in Benedikt's dictum, *tabicus nascitur non fit*—that is, that constitutionally certain victims of syphilis are predestined, as it were, to tabes—one would endeavour to discover such constitutional or hereditary predisposition, and take extraordinary precautions accordingly. The difficulty, however, is to ascertain beforehand the existence of such predisposition, for tabes may, in my experience, attack to all appearance vigorous specimens of manhood, and not those only who might be justifiably termed degenerates in any sense of the term.

3. As to the symptomatic treatment of tabes, I will deal with some only of the more common symptoms. The distressing lightning pains are notoriously favoured and intensified by cold and wet and atmospheric vicissitudes. Hence climate forms an important element in treatment. In my experience, sunny, dry, and uniform climates are especially suitable. To soothe the pains when they occur I have found nothing better than a combination of bromide with antipyrin or phenacetin.

Gastric crises are still more distressing and at times even dangerous, and in general require injections of morphine. The dangers of morphinism are, however, great, and I have seen a patient in the course of a year and a half increase his morphine consumption from 1 grain per diem up to 60.

In such cases Förster's heroic treatment by division of the posterior roots of the seventh to the ninth or tenth intercostal nerves seems a justifiable one. I have myself had no experience of this treatment, but the reported cases seem to show that it is effective and does not lead to any obvious evil after-results; and I should therefore be disposed to recommend it where milder measures fail. With a view to improve the muscular tone, which is so often impaired in tabes, and to which many of the characteristic attitudes and actions of tabetics are due, as well as to stimulate the sensibility, cutaneous and muscular, of the limbs, on which co-ordination so essentially depends, there is nothing better, in my opinion, than systematic massage and faradization, combined, perhaps, with hydropathy.

But the most important indication of all in the symptomatic treatment of tabes is to endeavour to restore the faculty of co-ordination which is failing by reason of the impairment of the kinaesthetic impressions; sharpening these by appropriate exercises, or compensating their loss by exercises under the direction of vision according to the methods laid down by Fraenkel. These are now so well known that they require no detailed description. The principles on which they are based command our approval, and the practical results are such as to repay their general adoption.

Dr. FEIBES (Aix-la-Chapelle) said that in by far the largest number of cases of tabes the disease was of syphilitic origin, and most probably all were of this origin, though this had not yet been definitely proved. He had

seen a number of cases in which a repeated specific course, alternating with tonic treatment, had produced indubitably good results, although not absolute cures. The patients regained full capacity for business. He also knew of patients who formerly suffered from severe gastric and laryngeal crises, but remained quite free from them for years after mercurial treatment. The same was true of ataxy and bladder troubles. His very experienced colleague at Aachen, the late Dr. Mayer, found among 202 cases treated with mercury about 3 per cent. almost completely cured, 20 per cent. very considerably, and 33 per cent. a little improved, and held that owing to refusal to admit the connexion between tabes and syphilis many patients suffered needlessly.[1] Experiences at Aachen showed that treatment with mercury and iodide proved harmless as a rule. The stupid fable that tabes was a consequence of too strong treatment with mercury and iodide had been rejected long ago, lastly by Neisser.[2] Courses of treatment must, of course, be administered prudently. In a large number of cases the insidious progress of the malady could not be stopped, but in no case did specific treatment prove harmful if carefully conducted. He did not like intramuscular injections in nervous diseases, and preferred inunction. If the mercury were given orally and were sufficiently energetically administered, digestive trouble would arise. Potassium iodide alone occasionally had a good effect in lessening the shooting pains. Injections of strychnine and thiosinamin seemed a favourable adjunct, but he had never seen any good result from silver nitrate. The shooting pains could be combated also with phenacetin, aspirin, and like drugs, and, if everything else failed, with morphine. Electricity and massage were of much assistance, especially if applied in the fashion recommended by Erb. Fraenkel's exercises were also a powerful factor in dealing with the ataxy. In conclusion, he would repeat what he had said at the International Congress in London in 1896:

> Therefore in all cases, if there is a history of syphilis, a mercurial treatment must be insisted on, especially if we are convinced that it will not prove harmful. I fully endorse Erb's opinion that the specific treatment of tabes will not be hurtful or aggravate the affection if there is no atrophy of the optic nerve. A properly conducted cure, even if extended over a long period, is almost always well borne, and it is just in cases of this kind that it has always appeared to me that the greatest improvement takes place after eighty and more rubbings. According to my experience, if any improvement shows itself, it is then that the treatment ought to be prolonged, and I see no reason advocating with Erb a stoppage after fifty to sixty rubbings more than any other number, provided that the patient is not affected by the treatment.

Dr. Lievin (Aix la-Chapelle) said: According to our modern views, tabes is not the direct product of syphilis, but is caused by the action of the antibodies elaborated by the body in response to the presence of the *Spirochaeta pallida*. Accepting this view, we can but conclude that the correct thing is to administer mercury in tabes, just as we would employ it as the recognized specific in more recent syphilis. Mercury has a direct destructive action on the virus as well as on the toxins produced; therefore, the treatment of syphilis which is commenced at a sufficiently early date in the disease to ensure the destruction or diminish the vitality of the microbe, thus decreasing the production of toxins, should prevent tabes. Secondly, if tabes be already present, it should be possible by mercurial treatment to arrest its further progress. To put it tersely and in a few words: "No spirochaetes, no toxins."

What I have just said is fully borne out by my personal experience. I cannot sufficiently emphasize this fact, that to prevent tabes a thorough and efficient treatment must be carried out in its entirety as soon as the diagnosis of syphilis has been made. In an experience now of twenty years all my 251 cases of tabes, with the exception of 11, give a history of no treatment whatever, or of insufficient, neglected, or inadequate treatment. I may mention that I consider treatment by the mouth as insufficient.

Coming to the second question with which we are concerned—namely, whether tabes, having clinically shown itself, can be favourably influenced by mercury, I must urge the difficulty of forming a definite opinion on this point, when one takes into account that no two cases are alike in their many manifestations, and that there are so many different ways of administering mercury—one method may be more effective than another giving good results, whereas another fails. I think my own experience will be borne out by my *confrères*, that, within the limits of anatomical possibility, no other remedy can replace mercury. These limits are always to be considered, and the physician cannot expect the replacement of lost nerve tissue any more than of a lost finger.

In all cases of tabes, especially in recently developed ones, a favourable influence is undoubtedly effected by mercury. In fact, as to early cases, I go so far as to say that the further development of symptoms can be completely prevented; and even in cases of a more pronounced type we succeed not rarely in doing the same, and even curing one or the other of the fully-developed symptoms as also mentioned by Dr. Risien Russell.

I invariably find great advantage and benefit derived from the inunction treatment when combined with hydrotherapeutic measures as we are in the habit of prescribing In some patients the shooting pains disappear for months or they lose their intensity; other cases get rid of their gastric symptoms, and generally in most the ataxy is improved. A frequent observation that I have made is that the quality or nature of the pain is changed from a lancinating to a duller or more bearable one. I find that I have notes from both lay and medical patients who had undergone a course of inunctions, telling me of this change in the character of their pains.

In my opinion, for tabetic cases the inunction method is the only suitable one. I have used injections of "grey oil" with far less gratifying results. These were in local patients at Aachen, where the patient could not be "rubbed" for social reasons. I know full well that good results may be obtained by injections of concentrated 40 per cent. "grey oil," but I must "drive it home" to you that a great risk is run of a sudden mercurialism resulting through the passing and absorption into the system of an old and unabsorbed dépôt of mercury in the gluteal region; this is quite unavoidable even by the most skilled operator. This accident, being sufficiently serious in the case of an ordinary syphilitic patient, may turn out with disastrous results in a tabetic, who is already quite run down and unbalanced.

On the other hand, with inunctions, such a dangerous degree of mercurialism can always be avoided by medical supervision. For at its first indication a good "soaping' and thermal bath will put a stop to further absorption and its dangers by at once removing the unabsorbed mercury on and in the skin.

Sulphur water taken internally, as well as sulphur baths at a proper temperature—which, by the way, must neither be too low nor too high—materially aid the absorption of mercury and lessen the risk of its employment. The chemical and other reasons for the use of sulphur water in combination with the inunction of mercury I cannot go into here.

In addition to the specific treatment, the necessity of constant medical supervision is a *sine qua non*. One's first duty is to maintain and improve the general condition and strength of the patient. If there should be a loss of weight, this is a definite indication to revise the method of treatment in regard to diet and exercise, and to employ the injection of tonic arsenical preparations, strychnine, fibrolysin, or lecithin, which I use freely. The economical expenditure of the patient's strength and power has especially to be considered if we prescribe in addition Fraenkel's exercises. The patient, if considerably ataxic, should be enforced to rest for prolonged periods, such rest to be interrupted by the prescribed exercises only.

Every voluntary movement, such as moving from one chair to another, should be undertaken as a task of the Fraenkel's system, the patient exercising control over each muscular act just as if he were carrying out his prescribed Fraenkel's exercise. Patients with wasting of the muscles of the legs I always get gently massaged.

Now as to the duration and general plan of treatment. The first course should be from seven to eight weeks. In the case of weak and debilitated patients, after having undergone a preliminary course of one to five weeks' treatment, there should be a rest, and treatment interrupted for a week or so and then renewed. With the continuance of treatment these interruptions become less or unnecessary.

[1] *Prognose der Syphilis*, Berlin, 1903.
[2] *Therapie der Gegenwart*, and Paul Cohn, *Berl. klin. Woch.*, 1903.

It is at these junctures that the physician must unswervingly persevere and insist on the continuance of the treatment, and to gain, above all things, the untrammelled confidence of his patient. This weakness and debility experienced by the patient can always be overcome by tact and a little "persuasive assertion." It has been the stumbling-block in the treatment of tabes, and the reason given that mercury does harm in this disease, but the gravamen lies on the medical attendant and not on the drug or remedy. Mercury does no harm to sensory nerves, its action being confined to the motor ones, as proved by all cases of chronic mercurialism.

If the tabetic cases be pronounced, a second "cure" should be commenced four weeks after the first, in milder cases after three to four months' interval. There should be a repetition of the cure after a year's interruption, which the patient is invariably pleased to undergo.

Dr. Judson Bury (Manchester), referring to the aphorism, "no syphilis, no tabes," pointed out that it was impossible to find evidence of syphilis in about 2 per cent. of cases of tabes, and that, as shown in an investigation by himself and Dr. Ramsbottom on the cerebro-spinal fluid, lymphocytosis was absent in about 15 per cent. of cases of tabes and general paralysis. Drs. Orr and Rows had shown that in the peripheral nerves, spinal roots, and cranial nerves there was a constant flow of lymph ascending to the central nervous system, the main current of which lay in the lymph spaces of the perineural sheath, and that toxins reached the spinal cord more readily by the posterior than the anterior roots. This important investigation suggested the necessity of looking for any peripheral septic focus—as, for example, cystitis in tabes—which might set up or aggravate lesions of the intramedullary sensory fibres. The speaker alluded to the difficulties attending the forming of an opinion with respect to the efficacy of any treatment in tabes, mainly owing to the temporary improvement, the arrested progress, and the partial recovery which were constantly observed in the natural course of the disease. He agreed with Dr. Russell as to the value of mercurial tincture treatment in tabes, and he hoped that in his published address Dr. Russell would, by the reports of cases, give ample evidence of the importance of this method.

Dr. J. Michell Clarke (Bristol) said: The question as to the treatment of tabes dorsalis necessarily depends upon the view taken as to its causation. If syphilis is accepted as the prime cause, we have to inquire whether the relation between the two is of such a kind as admits of successful treatment by antisyphilitic remedies, and, further, up to what interval of time after syphilitic infection is such treatment likely to be effectual.

It seems to me that the weight of evidence from clinical and cytological investigation is in favour of the view that syphilis is a necessary precursor of tabes, in spite of the fact that, like others, I have found it impossible to get a direct history or evidence of syphilitic infection in a certain percentage of patients. Again, like others, I have obtained a history of syphilis in a greater proportion of private than in hospital patients. The reason why patients ignore the fact of a previous syphilitic infection are various and have been fully discussed, and in my opinion are sufficient to account for the percentage of negative histories in tabes. It is, further, a point of interest as to how often patients had been infected with syphilis when held to be suffering from gonorrhoea only. Just as the percentage of syphilis is greater in tabetics in private than in hospital practice, so in those of my cases who denied syphilis a history of gonorrhoea was more than twice as frequent in private patients. A further point, about which more information is urgently needed, is whether patients who have been treated thoroughly for syphilis by mercurial inunction or injections develop tabes later in the same proportion as those treated by other methods, or in whom the disease has not been recognized. There is no doubt that persons who have been treated by mercury given by the mouth, carried out thoroughly for long periods, may subsequently develop tabes; in one case under my observation the lightning pains which were the early sign of the disease first began to show themselves towards the end of the three years during which the patient took pills of hydrarg. c cret.

A point of possible importance is that those who get syphilis young—say, before the age of 21—in my experience often develop tabes at a relatively shorter period after infection than persons affected at a later age. It is also worthy of remark that in my patients the age of onset of tabes in those who denied syphilis, possibly because, at any rate in some of them, the infection was so mild as to escape notice, works out at an average of nine years later than in those who admitted it.

With regard to the antisyphilitic treatment of tabes, I have carried it out hitherto, provided there are no other reasons to the contrary, in cases where other manifestations of syphilis were present, where the patient came under observation within about five years after infection, or where, with a rather longer interval since infection, the tabetic symptoms had developed rapidly, or where the patient had either been insufficiently treated for syphilis, or had had no treatment. In taking the history of these and similar cases, it is often found that many persons have not persevered with the treatment advised them for their syphilis; and this is especially the case in hospital practice. I should further take as evidence of active syphilis and of need for specific treatment the coexistence with tabes of signs of disease at the origin of the aorta in a person under 40 years of age. In cases in which there has been a long interval since infection, I have not hitherto tried mercurial treatment; nor in the class of cases in which the symptoms are mild, have come on later in life, and show little or no tendency to progress; in which, for instance, the chief symptoms are loss of knee-jerk, pupil signs, pains, slight disturbance of micturition, with, perhaps, Rhomberg's symptom, a slight degree of ataxy, or none at all. Many of these cases have suffered for years from lightning pains attributed to rheumatism or gout. I think further evidence is required to show that such cases would derive benefit from mercury, and from the often stationary character of the disease it is difficult to judge of the effect of any treatment.

Returning for a moment to the question of mercurial treatment in cases that come under observation at a long interval after syphilitic infection, one argument in its favour is derived from the results in women with tabes. In my experience, with, of course, a much smaller number of cases than in men, the results where it has been carried out have been favourable. This may partly be due to the difficulty of recognition of syphilis in women and consequent absence of treatment. Recently I have had examples of remarkable improvement in two women—in one ten years, and in the other eighteen years after the probable date of infection. They were treated by injections of mercury perchloride and by inunctions respectively. The latter was emaciated and cachectic, and the improvement in general nutrition was striking, but she was also under better conditions in hospital than at home.

I may add that, provided due care is used, I do not consider that anaemia and malnutrition contraindicate mercurial treatment; the result is often as in the above case.

Whenever I have thought mercurial treatment advisable, I have always employed the method of inunction. Although this mode of administration is by some authorities objected to as dirty and inconvenient, I have not found patients object to it as a rule, nor persist in such objection if the necessity for treatment is explained to them.

Quite lately I have also used injections of mercury perchloride without any trouble, and there is the advantage that the administration is less frequent. I have not, however, yet had sufficient experience to speak of the results. Lately the patients have been tested by Fleming's modification of the Wassermann reaction, but the number of cases is too small to be able to say anything about it as a guide to treatment.

With regard to the use of potassium iodide, large doses have not given good results in my hands, and I believe they may be distinctly prejudicial. Small or moderate doses continued for some considerable time are useful, especially in the more chronic class of cases, in diminishing the frequency and severity of pains, of gastric crises, of some of the late syphilitic sequelae, and in improving the general health. It is necessary to say, however, that it is extremely difficult in a disease like tabes, where there is so much variation in the severity of

the symptoms in the natural course of the disease, to judge of the results of any treatment by its effects upon symptoms. For example, treatment by suspension seemed to do good in many cases at the time when it was in vogue, but has now, I believe, practically gone out of use. Two or three of my old patients always insisted that nothing else was of so much benefit to them, especially in the relief of pain and of weakness of the bladder.

Difficult, however, as it is to estimate accurately the value of particular modes of treatment, there can be no doubt as to the efficacy of Fraenkel's exercises for the relief of ataxy. I should be inclined to put this in the first place amongst the advances that have been made in treatment of individual symptoms of tabes, and in it we have fortunately a remedy for that symptom which was formerly one of the most disabling and intractable. The severest forms of ataxy are met with more often in hospital than in private practice, and several times I have seen a patient who has been admitted to hospital bed-ridden and unable to walk or to stand without support, and, after four to six weeks' treatment by exercises, able to walk out unaided or with the help of a stick, and others, incapacitated by extreme ataxy, able to return to work. It is worth while to add that, although a well-equipped gymnasium is an advantage—elaborate apparatus is not essential—good results can be obtained with simple means.

In cases incapacitated and unable to take exercise from ataxy, arthropathies, or other causes, massage is also of considerable service.

In certain cases where the difficulty of getting about was aggravated by loss of sensation in the feet and legs, the application of a strong faradic current by the wire brush has brought about a considerable recovery of sensation, and the benefit was permanent. In others, this treatment failed to be of benefit; the reason for the difference I could not determine.

In symptomatic treatment, next to the advance in the treatment of ataxy by regulated exercises, or of equal importance with it, I should put the relief of pains by means of the modern analgesics. Of these, the best in my hands have been antipyrin and aspirin. The use of these remedies renders the employment of morphine rarely necessary, and enables one to reserve it for those very severe attacks of pain from which some unfortunate people suffer for two or three days at a time.

As a rule, the "girdle pain," though often constant and wearing, is not very severe, but sometimes it has been in my experience most intense and intractable to all forms of treatment, making the patient's life unbearable. In a persistent case of this kind, I am inclined to think that it might best be treated by division of the corresponding posterior root or roots.

It is unnecessary to speak of the importance of the care of the bladder in tabes, and of the early use of the catheter where there is retention or residual urine; but I may mention that such cases are occasionally complicated by infection with *B. coli*. In a recent case under my care the patient was taken profoundly ill with rigors, and a temperature reaching 104° to 105°; the condition was relieved by a vaccine made from cultures of the organism from the urine. I think it possible that this secondary infection with *B. coli* will turn out to be of not very rare occurrence in cases with bladder complications.

Lastly, whilst I think that the weight of evidence goes to show that syphilis is a necessary precursor of tabes, I also believe that there is some other agent at work adjuvant to the syphilitic poison, and that this may be of various kinds. I may therefore add a few words as to the great importance of maintaining the general health at the highest possible level, of putting the patient under the best possible conditions as regards habits of life, activities, and climate, and of looking after the state of nutrition. For this there is no doubt to my mind of the value of an occasional course of arsenic, and for the same purpose no remedy is more effectual than cod-liver oil taken regularly throughout the winter months.

Dr. E. Farquhar Buzzard (London) said: From personal experience of the use of mercury in tabes, I feel convinced that it is not only the most rational, but the most successful means at our disposal of staying the course and relieving the symptoms of that disease. No one would maintain that mercury or any other drug can restore to life neurones which are already dead. On the other hand, when tabes exists it is our duty to use such measures as are most likely to check the spread of the morbid process. In spite of the opinion expressed by some observers that antisyphilitic treatment is useless and sometimes harmful in parasyphilis, clinical experience has led me to believe that while the iodides are comparatively inactive, real benefit can in most cases of tabes be derived from the employment of mercury. If it occasionally fails, one has to remember that failure is not so uncommon when we are dealing with ordinary syphilitic affections of the nervous system by the same methods; and most of those present to-day must have seen, as I have, severe cases of cerebro-spinal syphilis go from bad to worse during the administration of antisyphilitic remedies. While agreeing with Dr. Russell that inunction is on the whole the most satisfactory method of giving mercury to patients suffering from tabes, I have not formed quite so poor an opinion of the results which can be obtained from giving the soluble salts by the mouth as he has expressed. In out-patient hospital practice this method is often the only one available, and I have seen most satisfactory improvement follow in many cases, even when no other form of treatment has been associated with it. I have now under my care patients who have been taking mercurial salts in this way for seven or eight years with short intermissions, and who have been relieved of many of their symptoms while continuing to carry on their work and supporting their families. It is usual to find that the administration of mercury by any method is attended by great improvement in general health; the body weight increases, the colour improves, energy is restored, lightning pains become very much less severe and sometimes disappear, and even trophic disturbances such as perforating ulcers rapidly heal. In endeavouring to explain the difference of opinion which undoubtedly exists as to efficacy of this treatment, I would emphasize the importance of persevering with it. One is constantly told that tabetic patients have had mercury at some time or other, and often that it has appeared to be of benefit; but the treatment has been discontinued, with the result that symptoms have returned in just the same way as they are certain to do in any case of cerebro spinal syphilis which does not submit to repeated courses of mercury for prophylactic reasons. When asked as to how long treatment must be continued, I can only answer that we have no means of deciding when it is safe to give it up, and that it is much wiser to go on repeating the courses indefinitely than to run the risk of allowing fresh exacerbations of the disease to present themselves. This statement is true of syphilis in all its manifestations, and great harm has been done in the past by the too arbitrary pronouncements of medical men that two or three or four years' treatment is sufficient to effect a cure. I agree with Dr. Russell when he states that the distance of time between the primary sore and the tabetic symptoms ought not to influence the doctor against prescribing mercury; and I would go further and say that the presence of optic atrophy is no contraindication to the use of that drug. In conclusion, may I suggest that the necessity for the use of numerous valuable remedies which we possess for the relief of the distressing symptoms of tabes would be very much diminished if patients suffering from this disease were universally and perseveringly subjected to efficient mercurial treatment, preferably by means of inunction?

Dr. Gordon Gullan (Liverpool) said: Undoubtedly the most important factor in the causation of tabes is syphilis; and I support Dr. Ferrier's view of "no syphilis, no tabes" as being strictly true. On that account most authorities have tried antisyphilitic treatment with great diversity of opinion; the majority appear to have obtained poor results, but others have found it useful in certain cases. Professor Erb believes that antisyphilitic treatment is of value in many cases, and that it is usually unattended with bad effects, but he admits that in a not inconsiderable number of cases it does not produce any good results. He advises it (1) in cases in which tabes has developed within a comparatively short time of the syphilitic infection; (2) in cases in which syphilitic symptoms or lesions are still present, or in which symptoms of cerebral or meningeal syphilis occur; (3) in cases in which the patient has not

previously had a satisfactory treatment for the syphilis. Collins recommends antisyphilitic treatment where tabes has developed within a period of five years of syphilitic infection.

During the past few years I have been trying the effect of regular intramuscular injections of mercury in cases of early locomotor ataxia over prolonged periods, and the results I wish to bring before you. The cases, seven in number—with two exceptions—were in an early stage of the disease—that is, the pre-ataxic. This is naturally the time when such treatment is most likely to be of any benefit. I will briefly record the cases and the results obtained:

Case I.

X. Y., aged 52, a member of a foreign diplomatic service, consulted me in September, 1907. He stated that for six months he had been suffering from shooting pains down the legs, but had been greatly benefited by mercurial intramuscular injections given him by a physician abroad. He had contracted syphilis more than twenty years previously. On examination I found a complete absence of knee-jerk, Argyll Robertson pupils, slight unsteadiness on turning round quickly or walking backwards, and slight anaesthesia in the lower part of his legs and feet. He complained, however, of the lightning pains, which, he said, had become much worse since he had been in this country, as he had not continued the treatment. The pains were so bad and persistent that his sleep was disturbed and he was unable to attend to his duties with regularity. As he had been having the soluble form of mercury, I gave him an intramuscular injection of mxx of a mixture of ℞ Hydrargyri perchloridi gr. viij, aquae ʒj, three times a week. The result was most marked, the pains at once diminished in severity, and within six weeks had quite gone; his knee-jerks gave a distinct response, and any little ataxia completely disappeared. Then, on the pretext of returning to the Continent, he gave up treatment, agreeing on my advice to continue it abroad. Three months later, however, he consulted me again for a return of his lightning pains, and I found him in the same state as when he first came under my observation. The same treatment was carried out with the same beneficial results, and this time it was continued for two months until he did actually depart for Italy, leaving me with the assurance of the continuance of it there.

Case II.

F. J., aged 39, a cashier, married, with no children, was sent to me in August, 1908, by Dr. Glover, of Waterloo, suffering from severe shooting pains down both legs, which were worse at night; knee-jerks were absent and the Argyll Robertson reaction present. When 20 years of age he had contracted a hard chancre, and had undergone a course of mercury for three years, and he does not remember any secondary manifestations. When he first came under my notice he had been troubled with lightning pains for the past thirteen months, which, with one intermission of six months, had steadily become so intense that he was obliged to give up work. His weight was 9 st. 9½ lb. Grey oil, mx (gradually increased to mxx), was injected into each buttock alternately every week, with the result that the pains immediately diminished in severity, and after five injections vanished altogether, and he slept undisturbed all night. After the first seven the injections were reduced to once a fortnight for three months, and since that time up to the present he has had injections every month or six weeks, and his condition has greatly improved. Any recurrence of the pains has been slight and of such rare occurrence that he has been able to work constantly. His nutrition is good, his weight has increased 7 lb., and slight knee-jerks can also be obtained.

Case III.

J. B., aged 47, merchant, consulted me three years ago for severe lightning pains which he called sciatica, and which were so intense that on several occasions he had been compelled to stay away from business. These pains were shooting in character and came on very suddenly, and had been present for a period of two months. He had contracted syphilis, but, as the symptoms had been slight, antisyphilitic treatment had been discontinued after a few months. His knee-jerks were sluggish, but no other symptoms of tabes dorsalis existed. I injected mxv of grey oil weekly for a period of eight weeks, and the pains rapidly subsided—in fact, were not at all severe after the third injection. His knee-jerks returned, and in the course of three months he was perfectly well, and he has had no recurrence. It is only fair to state that in addition I prescribed gr. xv of potassium iodide thrice daily. Although this is not a conclusive case of locomotor ataxia, yet, judging from the character of the pains, the reaction of the knee-jerks, and the history, it seems to me that this is not improbably a very early one of that complaint.

Cases IV and V.

V. Z., aged 42, dock labourer, and John S., aged 40, carter, both applied for treatment at the out-patient department at the Stanley Hospital. Each was in the pre-ataxic stage of tabes dorsalis, and gave a definite history of syphilis several years previously. Their symptoms were typical, and in both the shooting pains were the feature of which they most complained. In both cases grey oil was injected weekly with considerable benefit. Case IV steadily improved as regards the pain, and after three months' treatment ceased to attend the hospital. No improvement in the knee-jerk or Argyll Robertson pupil was detected. In Case V a similar improvement occurred, but, after having discontinued the treatment for three months, the pains occurred in their original severity, and he was obliged to undergo another course of injections. He has now been free from all pains for several months.

Case VI.

A. B., a seaman aged 50, came to the Stanley Hospital in January, 1909, complaining of severe shooting pains which prevented him following his occupation. On examination he was found to be suffering from locomotor ataxia, and mercurial injections of grey oil were administered once a week. In the course of three months his pains had altogether gone, and nothing more was seen of him until the end of April of this year, when he returned to the out-patient department, and requested that he could have some more of "them there" injections, as his pains had recently returned. During the interval of twelve months he had been quite free from all pain, and had been going to sea as an ordinary seaman. We found on examination that the pains were not as intense as formerly, and that a slight knee-jerk could be obtained, though when first seen this was completely absent. Since the renewal of this treatment his pains have steadily subsided, and he has now, after two months' treatment, returned to sea.

Case VII.

C. N., aged 40, baker, is the first case in which I have tried the treatment in a well-maked second stage of the disease, and I did so because of his complaining of very severe lightning and girdle pains. He has been under treatment for three months—April to June of this year—and has certainly greatly improved as far as the subjective symptoms are concerned, and, moreover, he feels and says he is very much better. His nutrition has improved, he has gained in weight, and his ataxia, which was very marked indeed when he first came to the hospital, has decidedly diminished. The anaemic condition of his blood also greatly improved under treatment.

Case VIII.

X. Y. Z., aged 65, a man engaged in a large wholesale business, was noticed by his doctor (Dr. Jackson, of Carnforth) in September, 1909, to be suffering from steppage gait, shooting pains, etc. He had a hard chancre when 30 years of age. He was sent to me a few weeks ago and was suffering from well-marked symptoms of the second stage of tabes, and was tremulous and hesitating in his speech. He was most anxious that he should not have what he called a "nervous breakdown," as he could not conveniently take his holiday till September, and last year he was obliged to be away from work for nearly three months. Although he has only had four injections, his nervousness of speech has greatly improved, his pains are better, and he has gained 3¾ lb. in weight. His general condition has correspondingly improved, his ataxia is distinctly less, and the anaesthesia of his feet and legs is not nearly so marked. He is now continuing his work with comfort and much greater ease.

The record of these cases, though of course by no means conclusive, owing to the difficulty of forming a true judgement of the effects of treatment in a chronic disease of this type, yet strongly support the opinion that the mercurial treatment of locomotor ataxia, if persisted in over a sufficiently long period, will give good results, and I would strongly repeat the view that Dr. Farquhar Buzzard has just expressed—namely, that mercurial treatment on tabetic patients must be long continued. I am confident that the more this is done, the better will be the results. When we consider the pathology of locomotor ataxia, especially in its early stage, when there is probably inflammatory thickening of the meninges of the spinal cord, I do not think that we need be surprised that improvement should take place. For the same reason I should like this line of treatment to be adopted in early cases of general paralysis of the insane. It stands to reason that the earlier mercury is administered in tabes dorsalis, the better will be the results. Hence the importance of recognizing the condition as soon as possible; and I believe that the excellent results obtained in Cases I and III were due to the fact that they were both seen and treated in very early stages.

Concerning the technique of the administration of mercury by intramuscular injection, I have followed the lines so ably advocated by Colonel Lambkin in the treatment of syphilis. It is important that the skin is carefully cleansed with ether and some antiseptic solution. The syringe (preferably a glass one with a platinum needle) must be carefully sterilized, and grey oil, if used, must be most carefully prepared, as bad results have been recorded from the mercury in this preparation not being sufficiently mixed and in a sufficiently fine state of subdivision. The posterior third of the buttock was invariably chosen as the site for injection, and each buttock used alternately.

With these precautions I have never seen any inconvenience beyond occasionally a little pain down the leg or at the site of the injection, but this has invariably passed off within twenty-four hours.

Frequency of Administration.—As grey oil is usually slowly absorbed, and its cumulative effects may be serious, I usually give it once a week for six to eight injections, then once a fortnight for two months, and then for once a month or six weeks for twelve months; after which it should be continued occasionally as the condition and convenience of the patient indicate. If the mercury perchloride injection is used there is not the danger of mercurialism, as it is more rapidly absorbed and equally rapidly eliminated, but it has the disadvantage that the injections must be given three times instead of once a week, and, as far as my experience goes, is more liable to be followed by a little pain.

Of course, attention must be paid to the condition of the teeth and gums as well as the heart and kidneys, just as in using any other form of mercurial therapy. Patients should be advised to use a mouth-wash of potassium chlorate or of hydrogen peroxide.

The great advantages of giving mercury by this method are:

1. Its *simplicity and cleanliness*, both for private and hospital cases.

2. Its more rapid and effective action. I am confident that the results appear quicker when the drug is given by intramuscular injection than when given by the mouth, and, moreover, gastric and intestinal disturbance is less liable to result.

As regards the method of inunction my experience is very limited, but it always appears to me that it must be difficult to regulate the quantity of mercury absorbed; moreover, in this country we have not the advantages of possessing the professional rubbers, who are so expert at Aachen and other places on the Continent. Moreover, intramuscular injections, when given with care, must certainly appeal much more readily both to the patient and his doctor. It is a form of treatment which can also be adopted in the out-patient department.

In conclusion, I would join with other speakers in urging that mercury, in whatever form, should be perseveringly persisted with in the treatment of tabes dorsalis, and am confident that if this is done the disease will not progress at the same rate or be held in such dread by patients and physicians.

Dr. Reginald Hayes (London) said that in cases of tabes, early tabes especially, his experience of mercurial inunction, well performed in combination with the use of sulphur water internally and externally, as administered at Aachen, was in many respects favourable, and bore out the views expressed by Dr. Risien Russell in opening the discussion. At Aachen some years ago, thanks to the courtesy of Dr. Feibes, he had the opportunity of seeing and interrogating many of these patients when under treatment, and subsequently in London he had employed precisely similar methods in a considerable number of cases. Most of the patients had previously taken mercury by the mouth for long periods, or had tried injection, auto-inunction, or the application of the drug by means of glass rubbers or gloves, the latter being the method in favour at Hot Springs in the United States of America. The results obtained—subjective and objective—pointed conclusively to the superiority of the Aachen methods. It should not, however, be supposed that Aachen treatment and treatment at Aachen were convertible terms, for the ritual there was carefully adapted to the needs of each individual case, and as a prolonged course was necessary, gentle handling was essential. One hundred rubbings, or, indeed, a greater number, were frequently required to obtain the best result. Improvement was usually slow in manifesting itself, and he had seen two cases of ptosis remain unaltered until seventy rubbings, and then completely recover. In a case kindly referred to him by Dr. Risien Russell, the knee-jerks returned in the ninth week. He had also observed gastric crises to cease under treatment. An interesting feature in several instances had been the marked improvement in the patient's ability to play games involving delicate co-ordination, such as golf and billiards. For many patients a visit to Aachen might not be possible, and for them the treatment could be carried out efficiently in London—Aachen methods being closely followed, and allowing for the difference of atmosphere between the pleasantly situated German Spa and that of our four-mile radius, the results compared very favourably with those obtained abroad. This was a point upon which he desired to lay stress, for he was frequently brought into contact with practitioners to whom it was a revelation.

Dr. Alexander Bruce (Edinburgh) drew attention to the position of the discussion. The problem before them was whether mercurial treatment acted upon the tabetic lesion or upon some syphilitic process which might accompany it. It would be admitted by all that where there had been untreated syphilis it was right to employ antisyphilitic treatment, as in a considerable proportion of the more rapidly developing cases of tabes there might be definite syphilitic meningitic disorder present. Treating the tabetic patient was not necessarily the same thing as treating the tabes.

Dr. Harry Campbell (London) found himself in full sympathy with Dr. Alexander Bruce's remarks. While admitting that all cases of tabes should periodically be put through a rigorous course of antisyphilitic treatment, whether in the form of mercurial inunction or injection, in order to remove any tertiary effusion that might be present, he much doubted whether such treatment touched the actual disease, which, when once started, tended, in spite of any treatment hitherto suggested, steadily to progress to the end of the chapter. In estimating the effect of mercurial treatment, many factors had to be taken into consideration. Cases of pseudo-tabes from tertiary lesions naturally benefited by it. It had further to be remembered that many of the symptoms of tabes were largely functional, in the sense that they were not constant, but varied from time to time, or even entirely disappeared. That was the case, for example, with the urinary and sexual disturbances, the ataxia, and, above all, the lightning pains. Such symptoms were influenced by climate and suggestion quite independently of mercurial treatment. They could also be benefited by local mechanical means, whether reflexly or by suggestion could not be said. An American physician had suggested that many of the symptoms of tabes were kept up by the irritation caused by urethral stricture, and that could be improved by dilating the stricture. This treatment had been adopted recently in three of the speaker's patients under the supervision of Dr. Jensen, and the results were quite as striking as any which had been recorded that morning. There had, for example, been marked alleviation of pains, ataxia, and gastric crises, and one patient had put on weight. In the light of this experience, the speaker could not help wondering whether the rapid effects which had been mentioned as resulting from mercurial injections might not have been equally well obtained by injecting distilled water.

Dr. Risien Russell, in his reply, said that he had purposely limited the scope of the discussion to whether or not mercurial treatment was of advantage in tabes, for the reason that it was impossible to hope to overtake the whole of the treatment of the affection in the time allotted for the discussion. He was glad that he had received such strong support, both in regard to the syphilitic origin of tabes and as to the efficacy of mercurial treatment in the disease. At least 19 per cent. of the 20 per cent. of cases in which it had been claimed that the patients had not had syphilis could be disposed of by the fact that patients commonly denied syphilis when questioned by the family doctor, while they admitted the truth when they sought the advice of the consultant. While gratified to receive so much support in his advocacy of the inunction treatment from those most competent to speak on the subject, he wished to repeat that he did not feel himself competent to contrast the inunction with the injection treatment, for the reason that he had obtained such good results from the former that he was loth to abandon it for the latter. What he did wish to insist on, however, was the superiority of either of these methods of treatment to that of administering mercury by the mouth. He welcomed the criticism that no time limit should be placed as to how long the treatment should be continued, but thought it well to fix some such limit as he had

suggested when advising patients, as otherwise it might be difficult to persuade them to undertake the necessary cure. Age was no necessary bar to the treatment, for he had seen excellent results in people who had passed the middle period of life. When the treatment was properly carried out the general condition of the patient was favourably influenced, so that they commonly improved in colour and put on flesh. In selecting such phenomena as return of the knee-jerks and of the pupil reaction to light, he did not mean it to be understood that these were the only ways in which patients so treated showed improvement. In reality, pains, crises, ataxy, and sphincter affection all improved in a notable manner under this plan of treatment. It might, however, be said with reason that all of these symptoms were known to improve in some cases in which the mercurial treatment had not been carried out, so that he preferred to quote return of the knee-jerk and of the pupil reaction to light as the best evidence that he could adduce in favour of the view that mercury influenced the lesions of tabes for good, as no other plan of treatment could claim such results as these. There were, of course, many useful adjuncts in the treatment, to which reference had been made by some of the speakers, but it was impossible to pretend to deal with them in the time at his disposal. In conclusion, he regretted that any one should have introduced into a discussion of the kind the suggestion that dilatation of the urethra was capable of affecting tabes in a favourable manner. His only experience of this treatment was in a case in which the tabes had evidently been made and cured by those who had so "treated" a patient in whom, to his own knowledge, there had never been the slightest indication of the disease for which the patient had been so treated, and of which he was persuaded that he had been cured.

ON THE PSYCHOLOGICAL TREATMENT OF CERTAIN FUNCTIONAL CONDITIONS.

By EDWIN ASH, M.D.Lond.,
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THERE has lately been considerable discussion at various meetings of the medical societies as to the propriety and value of using psychological methods in treatment, particularly in cases where disordered health appears to be dependent on some functional nervous disturbance. And as I have myself devoted much time to the investigation and practice of psychological methods of treatment, perhaps my experiences may be of interest to others who wish to investigate this particular aspect of therapeutics.

My own interest in mental methods of treatment was aroused when as a student I saw the remarkable effects of such factors as hope, encouragement, confidence, and personality, in helping people to throw off their ailments; and since then I have never ceased to examine the possibilities and practical application of mind influence in medicine.

In this connexion I was led to investigate the question of the hypnotic sleep state, but although I found it a remarkable agent for psychological experiments and analysis, on applying it to medical work I found that its uncertainties were such that it did not appear worthy of the place in medicine which some authors have endeavoured to obtain for it. So that when my investigations were directed from the experimental aspect of the influence of suggestion to its therapeutic application, I endeavoured to confirm the observations of numerous contemporaries that suggestive therapeutics can be successfully practised without the induction of hypnotic sleep. And the results obtained have shown me that by the careful application of a certain technique direct suggestion will do much to relieve a large number of functional disorders. Indeed, it is remarkable to find how pain and hyperaesthesia of special senses can be alleviated by a simple system of suggestive therapy, in the application of which one's patients are conscious of all that is being done, and, moreover, retain perfect liberty of thought and action.

Suggestion in Practice.—With regard to the actual application of suggestion in treatment, the practical technique to be followed must be carefully varied according to circumstances. In its simplest form suggestion may be brought to bear on a patient by placing him in a comfortable chair, telling him to close his eyes and relax every muscle as far as possible; all distracting noises should be avoided, and the patient instructed to fix his attention solely and earnestly on what is being said to him by the physician, who, in a quiet, persistent voice enumerates the particular symptoms for which relief is sought, and confidently gives reasons why these will shortly disappear, and assurances that they will undoubtedly do so.

It must be remembered that under ordinary conditions of life the mind is a sort of kaleidoscope of ever-changing ideas and thoughts, few of which attain great intensity of ideation. Impressions are accepted and fade away as they are replaced by others. We know that if we have to think out some difficult problem in diagnosis we want to be left alone for a while—we want other people to stop talking to us, and distracting sounds to be shut out—so that by withdrawing our attention from all outside impressions and wandering thoughts we may be able to *concentrate* our attention on the points in question.

Supposing one is making a digital examination of some cavity of the body—I will mention rectal and vaginal examinations as examples, for they are part of the routine work of practitioners—is it not a usual thing to fix the attention by gazing steadily at some distant object, and, by freeing the mind from disturbing impressions, to allow it to concentrate on the sensory impulses that are being sent up to it from the finger-tip?

Hence when we wish to present with force a *curative suggestion* to the mind of a patient, we must endeavour to quiet his thoughts, and protect him from the disturbing influence of impressions coming in from without. An orator trying to make a speech in a roomful of talking people will not make very much impression; but when his audience has been quieted, the ideas he presents will have some definite weight. In suggestive therapeutics the physician is the orator, and the restive audience is represented by the turbulent thoughts of his patient.

Now to obtain the necessary calm, receptive condition of the patient's mind, it is in practice found best to let him rest in a perfectly comfortable restful condition, so that no stimuli from skin or limbs will disturb him; to get him to close his eyes, so as to shut out visual impressions; and similarly to have the room as quiet as possible so as to avoid disturbing auditory impressions.

Then we shall have only to deal with thoughts and ideas arising from within; but as a matter of fact, when any one is resting comfortably with their eyes closed and in a quiet room, thoughts always tend to quieten down, and with many people definite drowsiness quickly comes on. In such a state of mind the patient is ready and receptive for any idea that may be presented to him, and one endeavours by repetition to implant curative suggestions which will take root, as it were, and manifest their beneficial effect even when his mind resumes its usual activity.

As one would expect, functional diseases particularly lend themselves to treatment by suggestion, because they appear to be invariably dependent on a mental factor, especially those very common functional neuroses which come under the headings of neurasthenia and hysteria. Indeed, as the leading factor in the treatment of neurasthenia and hysteria, I have found suggestion to be more reliable in its effects than any other principle, and I have had a good many cases through my hands who have improved very quickly under its influence.

Enuresis Nocturna.—I should like to emphasize the fact that in the treatment of the functional incontinence of urine so commonly met with in children and adolescents suggestion is remarkably successful.

Among the first cases I treated by suggestion were two of functional enuresis nocturna. The first of these was a girl, 15 years of age, who was unable to remain in service owing to the unpleasant nature of her affliction. She had fourteen sittings during three months, suggestions being given that she should awaken with intent at a certain time each night. There was definite improvement after the third sitting, and complete recovery after the tenth. At no time during the treatment was the patient unconscious. The other case of this description was also relieved, and was very interesting, because I first treated him in the hypnotic sleep state without result. So I tried suggestion in the waking state with fixed attention, and obtained immediate improvement, with ultimate cure. The condition of these young people with enuresis

nocturna is most distressing, and if no organic cause can be found it seems desirable, after a consideration of such results, that they should be given the benefit of treatment by suggestion.

In cases of drug habit, particularly in morphinism, suggestion is a most potent help in enabling patients to recover their self-control. I have had successful results in cases of morphine habit, but have never undertaken the treatment of alcoholism by this means. Others have reported very good results from the use of suggestion in cases of alcoholism.

In mental disturbance, not amounting to actual insanity, I have found the treatment give very good results—better than, I suppose, could be expected from any other method. In several instances I have found auditory hallucinations quickly yield to direct suggestion; these to me, I must confess, were surprising as well as gratifying results.

Where there is much depression of mind I have invariably found the treatment had to be persevered in for a much longer time than otherwise when one is dealing with states of mental excitement. Still, I have had encouraging results with melancholic patients, and have seen apparently hopeless cases recover speedily under the influence of suggestion. I remember one case in particular—that of a young lady who developed extreme depression with suicidal impulses—who reacted in a remarkable manner to the treatment by direct suggestion, and has had no attack since—that is, during the last three years.

In cases of sleeplessness where the condition is not due to painful organic disease or to circulating toxins, the method of treatment I have outlined is usually productive of excellent results; and I most strongly recommend it in all cases where insomnia of a functional kind is prominent.

Similarly, direct suggestion will relieve headaches and neuralgic pains of various kinds when these are not directly due to organic disorder. And other functional conditions in which I have personally used the method with success include dyspeptic symptoms and neurotic disorders of the heart, chiefly of the nature of what is popularly known as "palpitation"; various kinds of muscular twitchings and spasms, notably cases of severe blepharospasm; painful scars; dominant ideas and obsessions; and aphonia.

I find that constant practice of psychological treatment and increasing experience of the cases which it will relieve is, of course, a great help to success, and I look forward to the time when these methods of treatment will be more generally used in our profession than they are at present.

THE KNEE-JERK AND SIMPLE REFLEXES.

By W. A. Jolly, M.B.

(From the Physiological Laboratory of the University of Edinburgh.)

The object of the research, which was undertaken on the suggestion and with the help of Professor Schäfer, has been to record the interval of time elapsing between a tap on the patellar tendon and the beginning of the electrical variation indicating activity of the quadriceps muscle, and to compare this interval with the delay occurring in the case of reflex action of the thigh muscles in response to mechanical stimulation of the skin of the foot.

The knee-jerk has been timed in the human subject, rabbit, intact and decerebrate, the decapitate cat preparation and "spinal" cat, and comparison has been made in the decapitate and spinal cat with the homonymous flexion reflex of the hind limb and the heteronymous extension reflex. The latent period of contraction of the quadriceps has also been measured in the decapitate cat when the anterior crural nerve is stimulated by a mechanical tap close to the muscle.

The records have been made in the following manner: In the case of the knee-jerk a piece of rubber covered on the upper surface with silver foil is attached to the skin over the tendon. A light metal hammer falls upon this, producing the stimulation and at the same time closing the circuit of an electrical signal. The wicks of two non-polarizable electrodes are held in position by rubber bands upon the shaved skin over the thigh muscles, and the electrical variation recorded by means of Einthoven's string galvanometer. The movements of the string and of the signal, which consists of a small string galvanometer, are recorded together upon a photographic plate, which is shot by a spring past a horizontal slit at a rate of 850 mm. per second. The shadows of a tuning-fork giving 120 double vibrations per second and of the teeth of a revolving disc are also thrown upon the slit.

Measured in this way, the time of the human knee-jerk, as determined in three subjects, ranges from 18.6 to 23.7 thousandths of a second (σ). In the intact rabbit the time varies from 5.2 to 6.6 σ; in the decerebrate rabbit from 6.0 to 7.8 σ; in the spinal cat from 5.3 to 7.9 σ; and in the decapitate cat preparation from 6.2 to 12.4 σ.

The homonymous and heteronymous reflexes of the hind limbs are elicited by attaching to the dorsum of the hind paw a vulcanite plate, bearing several needle-points on the lower surface, and covered on the upper surface by silver foil. The stimulus is given by the falling hammer, which, striking the plate, drives the points into the skin and closes the signal circuit. The non-polarizable electrodes are placed upon the skin of the thigh over the adductor and extensor muscles.

The following figures were obtained in one experiment on the spinal cat, where the knee-jerk and homonymous reflexes were elicited alternately:

Knee-jerk.				Homonymous Reflex.
(1) 6.87 σ	...	...	...	(2) 13.18 σ
(3) 6.83 σ	...	...	...	(4) 12.36 σ
Mean 6.85 σ	...	...	...	12.77 σ

The averages of 12 observations on two spinal cats are: Knee-jerk, 6.62 σ; homonymous reflex, 12.75 σ.

The averages obtained in a series of observations on a decapitate cat preparation are: Knee-jerk 8.49 σ, and homonymous reflex 16.75 σ.

The heteronymous reflex was studied in the spinal cat, and the times obtained range from 11.9 σ to 19.9 σ. The delay was found to be long soon after the cord was cut. It progressively diminished until, three weeks after section, the times of homonymous and heteronymous reflexes were closely alike.

The delay in the knee-jerk is found to be more variable when the decapitate preparation is used than in the case of the spinal animal. Nine preparations have been employed in the investigation, and while the shortest times obtained are similar to those given by the spinal animal, intervals twice as great have also been observed.

For the purposes of measuring the latency of muscular contraction in decapitate preparations, after the knee-jerk has been measured the anterior crural nerve is exposed and cut 2 cm. above the muscle. The peripheral end is placed between two vulcanite bars, the upper of which bears a metal plate, and is struck by the falling hammer. The latent period obtained in this way ranges from 2.2 σ to 3.2 σ.

The following figures were obtained in two experiments:

1. Knee-jerk, 12.4 σ; latency on nerve stimulation, 2.2 σ.
2. Knee-jerk, 10.5 σ; latency on nerve stimulation, 2.7 σ.

Here the latency of the jerk exceeds that of the muscle stimulated through its nerve by 10.2 σ and 7.8 σ respectively.

The fact that the time of the knee-jerk may sometimes be so lengthened while the muscle itself responds to nerve stimulation in a much shorter time, together with the well-known dependence of the jerk upon the reflex arc, raises anew the question as to whether this phenomenon can be regarded as due to direct stimulation of the muscle. At the same time we must bear in mind that the time required for a simple reflex, such as flexion of the thigh determined in the same animal, is found to be, roughly, twice as long as for the knee-jerk. The conclusions of previous observers, that the time of the knee-jerk is much shorter than that of such a simple reflex, are therefore borne out by this research.

In order to ascertain whether there is sufficient time for nervous conduction to the spinal cord and back to the quadriceps muscle, the animals used were dissected and the distances measured from the patellar tendon along the muscle and anterior crural nerve to the cord and back to the position of the proximal electrode which lay over the upper end of the vastus internus.

Piper[1] has recently determined the rate of conduction in human nerve to be from 117 to 125 metres per second. Assuming this rate to be applicable to the animals experimented with, we must deduct from the times found for the knee-jerk in spinal and decapitate cats a period of about 2σ, varying with the size of the animal, for nerve conduction. We are left with a period of 3.32σ in the case of the shortest time observed and 10.1σ in the longest, which, if we regard the knee-jerk as a reflex, would include the time occupied by the impulse in its passage through the central nervous system.

In the case of the homonymous and heteronymous reflexes the nerves have also been dissected out and measured, and the time occupied in nervous conduction calculated at the same rate. We have, then, for the averages in the spinal cats—knee-jerk, $6.62 - 2 = 4.62\sigma$; homonymous reflex, $12.75 - 3 = 9.75\sigma$; and it will be observed that the latter remainder is almost exactly double the former. In cases where the determination of knee-jerk and flexion reflex are made alternately and rapidly, the duplication is still more exact—for example: Knee-jerk, $6.85 - 2 = 4.85\sigma$; homonymous reflex, $12.77 - 3 = 9.77\sigma$.

A period of 3.32σ appears to be very short to include passage of an impulse through the spinal cord, but, as we have seen, this period may be increased to as much as 10.1σ—doubtless owing to unfavourable conditions of circulation, temperature, etc., obtaining in the decapitate preparation—a figure which exceeds the remainder left after deducting conduction time in the flexion reflex.

It is difficult to explain times of 11 and 12σ for the knee-jerk when the latency of the muscle at the same time and under the same conditions to stimulation of its nerve can only account for 2 or 3σ, except on the hypothesis that we are dealing with a reflex whose path in the spinal cord is simpler than that of the flexion or crossed extension reflexes. It may be—and the ratio of the periods lends support to the view—that the knee-jerk mechanism involves one spinal synapse or set of synapses, while the other reflexes mentioned involve two.

REFERENCE.
[1] *Pflüger's Archiv*, Bd. 124, S. 591, 1908.

DEMONSTRATION ON SCLEROSIS OF THE ADVENTITIA.

By A. BRUCE, M.D.,
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DR. ALEXANDER BRUCE gave a lantern demonstration illustrative of a sclerosis of the adventitia of the vessels of the spinal cord, associated with multiple (amputation) neuromata in the substance of the cord. The specimens were obtained from a patient who had been under the care of Dr. Affleck in the Longmore Hospital for Incurables, Edinburgh. She had died at the age of 30. Her illness had commenced apparently about the age of 7, with "water in the head." About the age of 10 weakness of the lower extremities began to develop, followed by muscular wasting and loss of the knee-jerks. The weakness gradually extended upwards to the trunk and upper extremities, and the patient ultimately became bedridden.

Pathological examination of the cord showed that the condition had apparently begun as a sclerosis of the membranes in the neighbourhood of the points of emergence of the anterior and posterior roots, from which points it had spread along the membranes, and had entered the substance of the cord by the lymphatic system of the adventitia of the vessels, and had extended along these vessels irregularly as far as their ultimate terminations, producing a great thickening of the outer wall with a connective tissue. The result was a series of areas in the grey and white matter differing from those of typical disseminated sclerosis in this respect, that the patches of sclerosis contained actual fibrous tissue which had grown in from them, and were not due to mere overgrowths of the neuroglia.

In addition, the lower portion of the cord, and, to a lesser extent, the cervical region, showed numerous microscopic tumours, nodular or spindle-shaped or irregular, situated in the antero-lateral part of the cord and its membranes. The tumours were composed of spindle-shaped cells associated with medullated nerve fibres. In every instance small tumours were situated in the adventitia of the blood vessels. The connexion of these nodules could be traced along the adventitial cells of the blood vessels outwards to the pia mater, and along this as far as the anterior roots, from which they were clearly derived. It appeared probable that the neuromata were simply secondary results of the deflection of some of the anterior root fibres by the sclerotic tissue; that these aberrant fibres had grown in the lines of least resistance along the membranes, entering the substance of the cord by the lymphatic system and the adventitia of the vessels, and passing inwards until their growth was sufficiently resisted, whereupon the neuromatous tumours were formed, of essentially the same character as those formed in a scar after amputation.

Memoranda:
MEDICAL, SURGICAL, OBSTETRICAL.

HAEMORRHAGE OF THE NEWBORN.

A STRONG, healthy male child, 3 days old, sleeping quietly by the side of its mother, began recently in the early hours of the morning to vomit blood. This coming on without any warning or apparent cause naturally alarmed the parents, and they sent for me. Later on the child passed blood by the bowel, some of it gushing out and passing through the diapers. The child was blanched and anaemic, and a very unfavourable prognosis was given. The haemorrhage from the bowel kept on for the next two days, and then ceased as suddenly as it had begun. No treatment by drugs of any kind was undertaken, the only thing being that the nurse was directed to keep the child warm and as quiet as possible. The child took the breast well all the time the bleeding was going on. There was no idea of the child drawing the blood from the breast or of injury at birth, and there were no symptoms pointing to ulcer of the stomach or bowel. There was no jaundice. The child is now healthy and well. I am unable to give any reason for the haemorrhage and the pathology seems equally obscure. The rarity of this haemorrhage in the newborn perhaps justifies the publication of the case.

Dalton-in-Furness. T. F. FORSTER.

CASE OF LINSEED POISONING.

I WAS called to see a boy, aged 13 years, by an urgent message one morning, the parents suspecting scarlet fever.

When I saw the boy he was acutely ill and distressed. His arms and face were covered with a red flush and with some papular eruption scattered about. The face and eyelids were swollen and oedematous. He had vomited once. On investigating the rest of the body the diagnosis was apparent as acute urticaria from some cause. The upper part of both back and chest was one huge wheal raised about $\frac{3}{16}$ in. and of a yellowish colour, the wheal of the back extending from the neck to the lower dorsal region. There were numerous small wheals and patches all over the rest of the body and legs; the tongue and fauces were swollen and red. I saw him at 1.40 p.m. Questioning him and his parents as to ingested materials, etc., the boy admitted having eaten a piece of linseed cake as big as a barcelona nut at 11.30 a.m. The red rash and discomfort began to appear at 12.30 p.m. He vomited once at 1 p.m. With calomel and salines he was all right the next day.

H. B. WILLOUGHBY SMITH, M.B., F.R.C.S.

Gainsborough.

THE presidential address at the opening of the twenty-ninth session of the West London Medico-Chirurgical Society was delivered by Dr. Phineas Abraham, who dealt with specialism. He regarded this as an inevitable outcome of the rapid advance of science, but not one which in any wise made the work of the general practitioner unnecessary. On the contrary, the general practitioner, with his wider study and experience of disease, could greatly advance medical knowledge if he would record facts that came under his notice. He could help the specialist by testing the results of his work, just as the specialist could in his turn help the general practitioner by dealing with particular cases.